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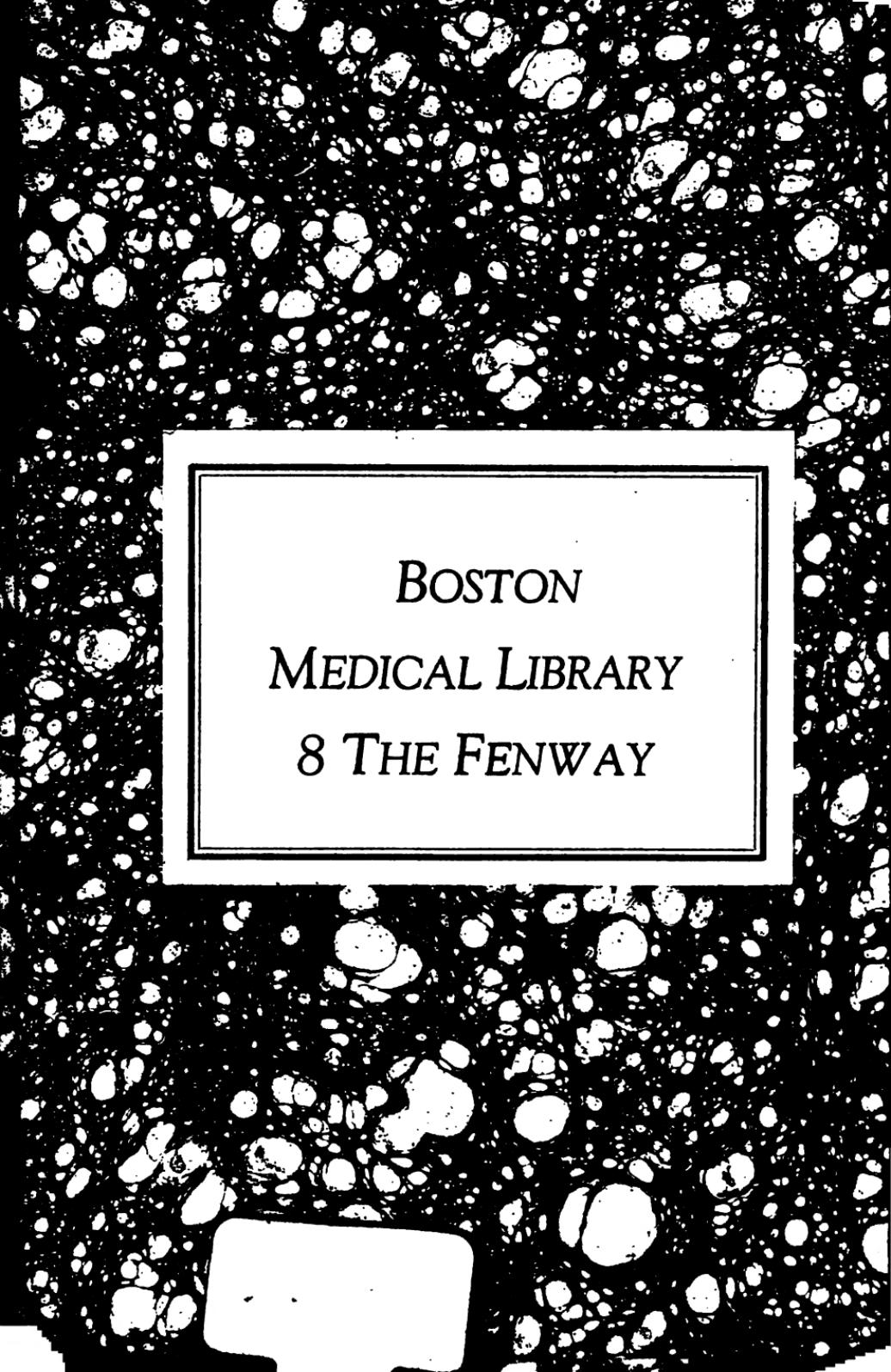
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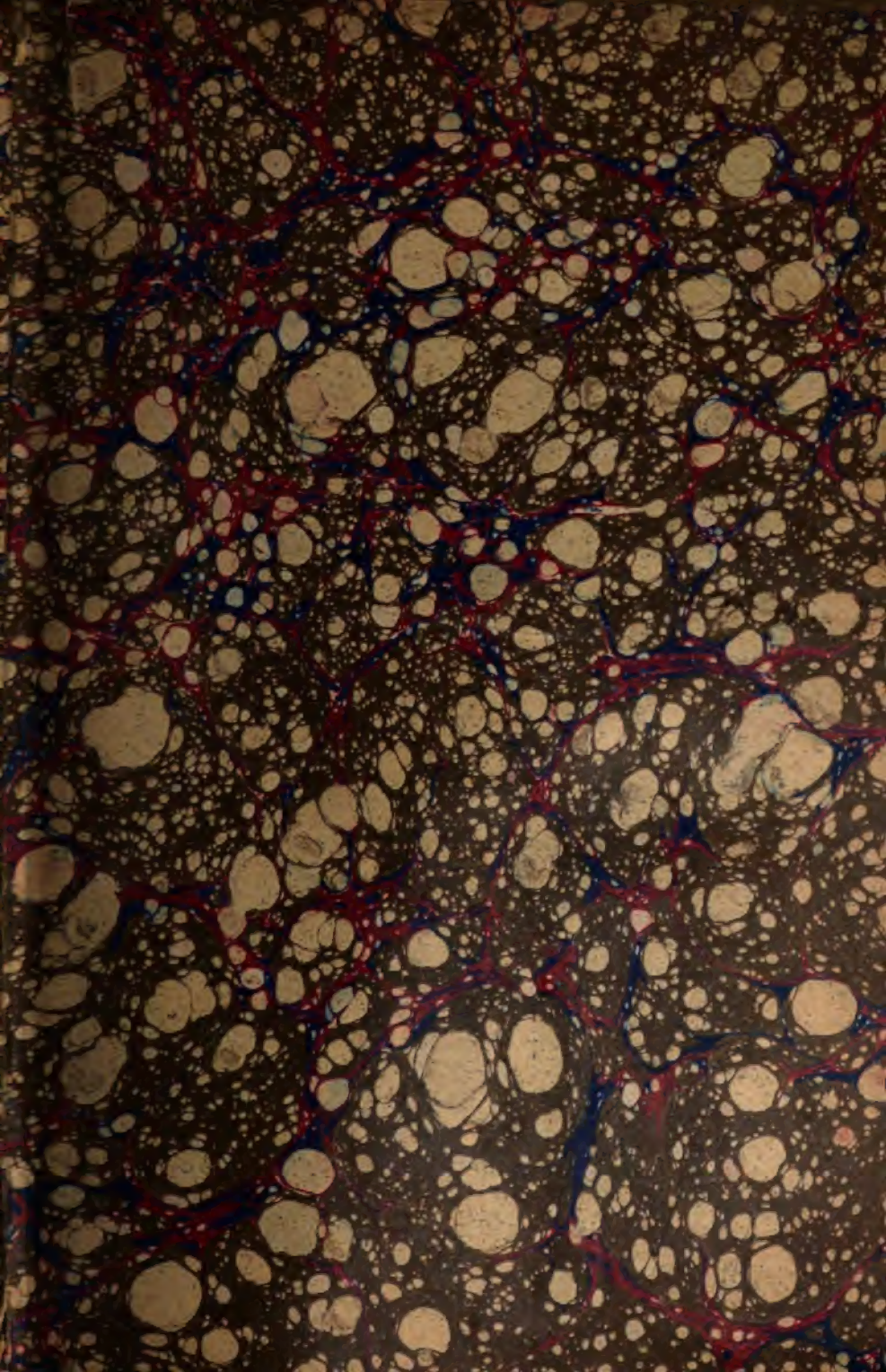
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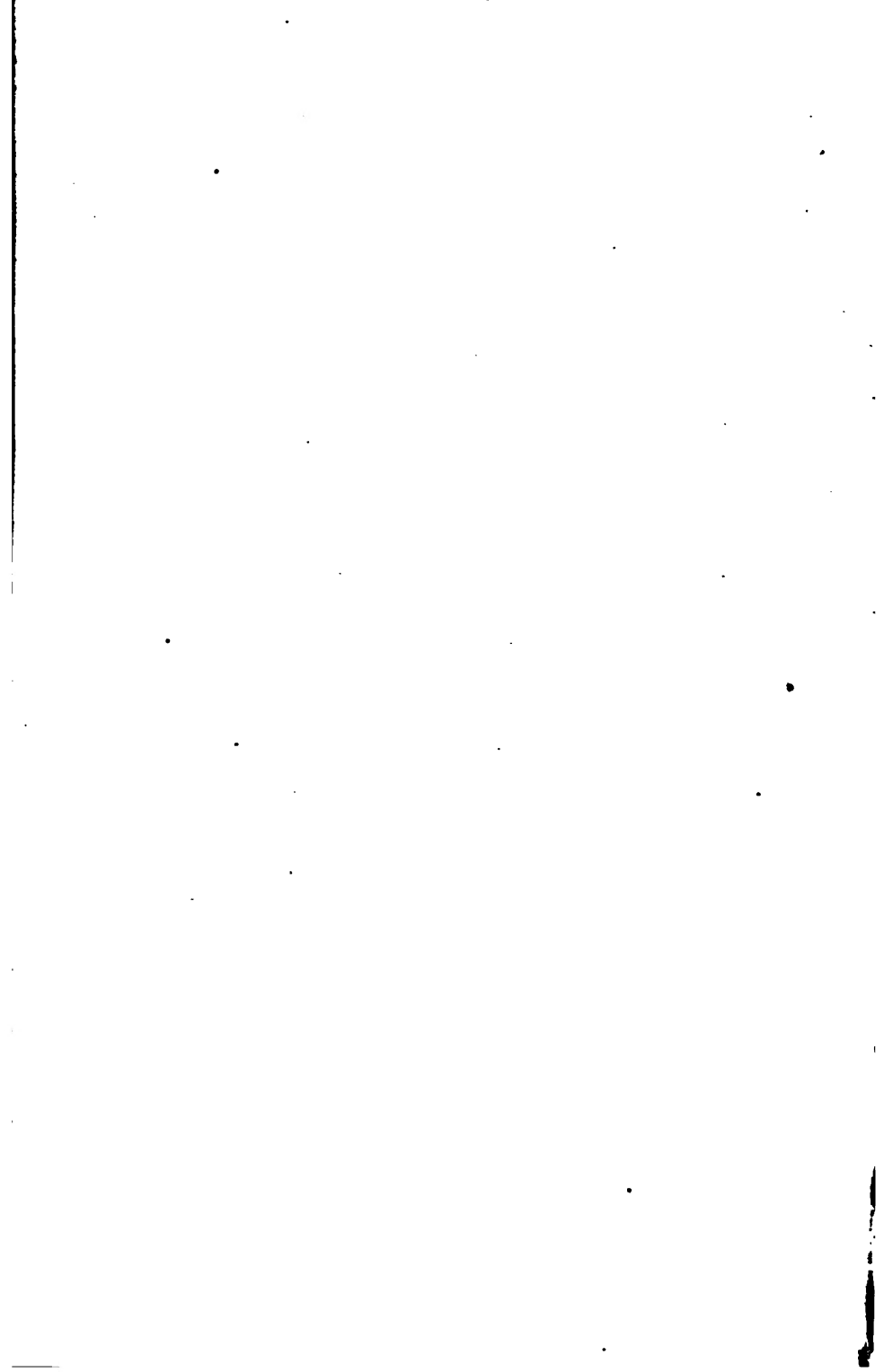
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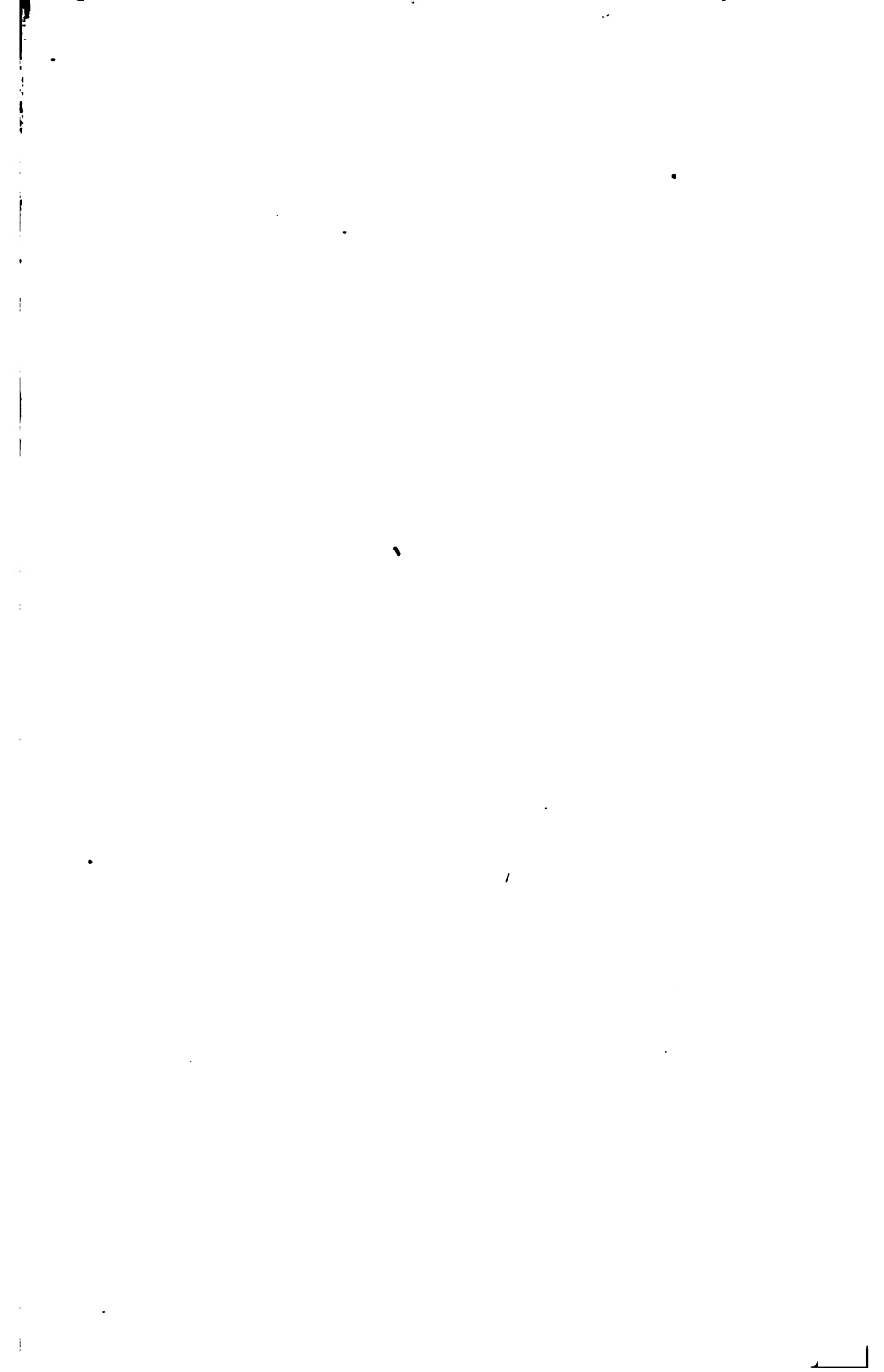
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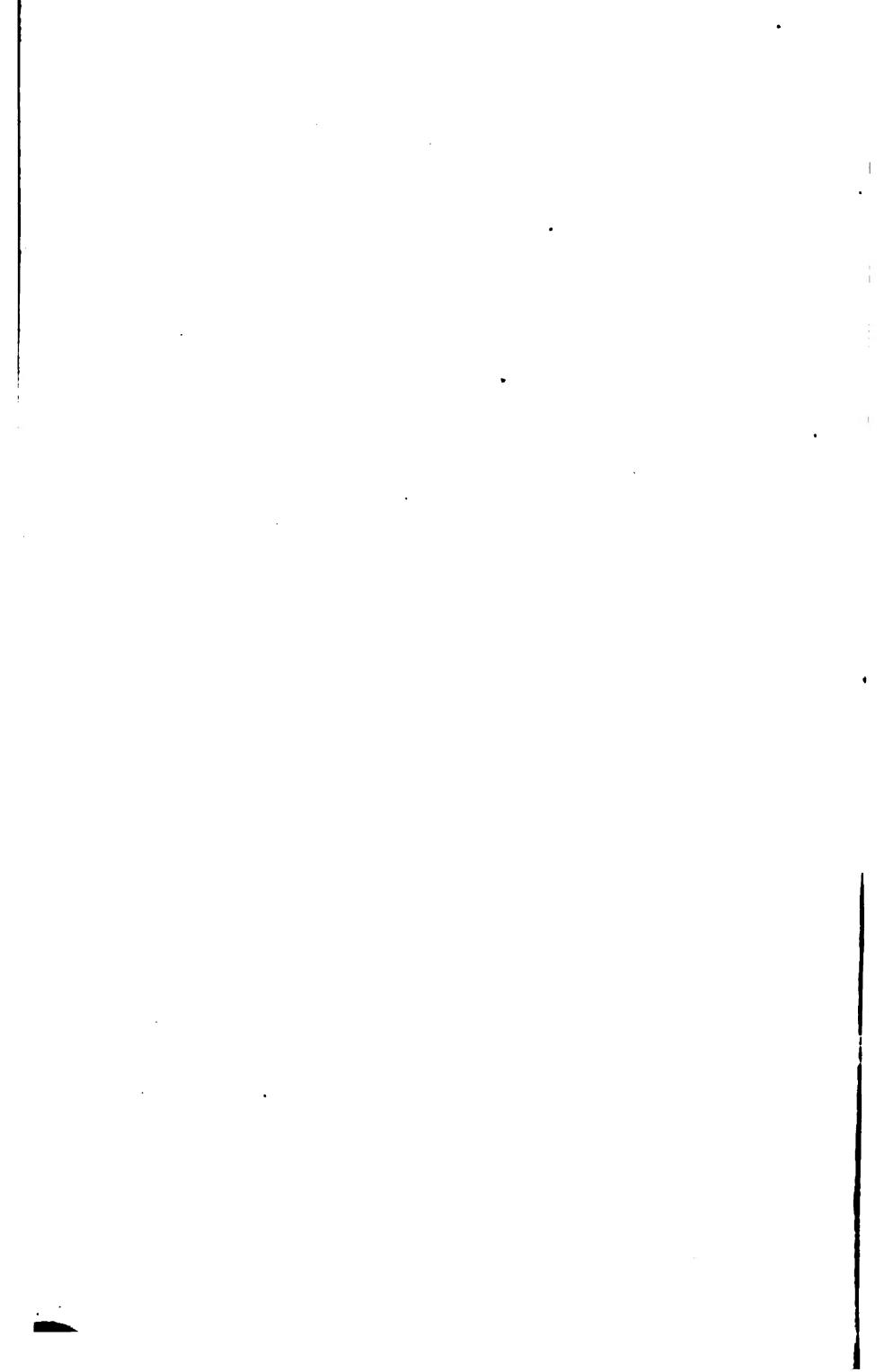
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THE PRACTITIONER:

A JOURNAL

OF

THERAPEUTICS AND PUBLIC HEALTH.

EDITED BY

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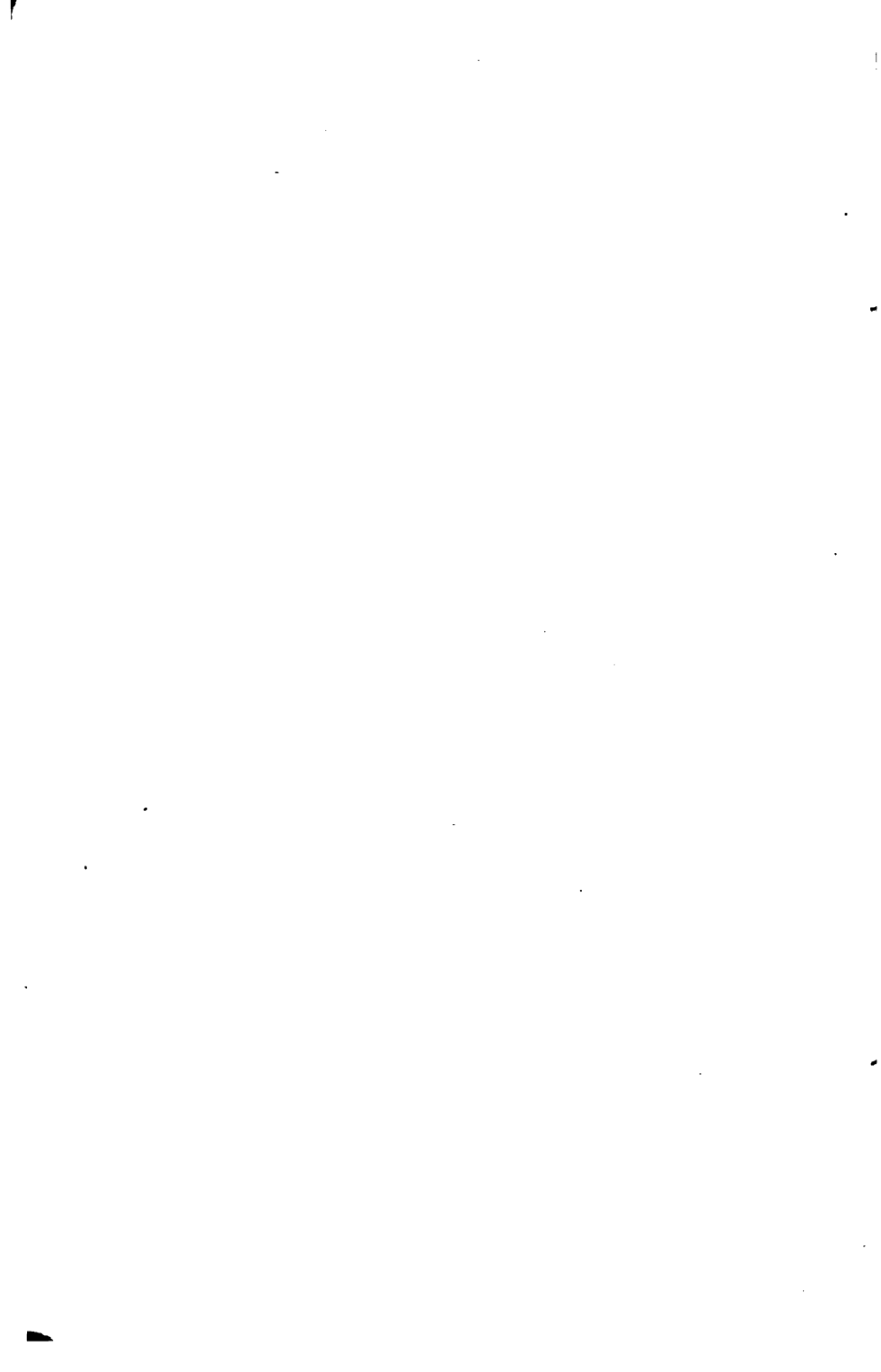
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THE PRACTITIONER.

JULY, 1873.

Original Communications.

"WRITER'S CRAMP:" ITS PATHOLOGY AND TREATMENT.

BY G. V. POORE, M.D.,

Assistant Physician to Charing Cross Hospital.

PART II.

WE have assumed, in accordance with recognised physiological principles, that the functional activity of an overworked muscle is impaired. Muscles thus abused become tired out, and degenerate into a condition of *chronic fatigue* or *irritable weakness*; and we shall see, on comparing chronic fatigue with acute fatigue, that the symptoms which accompany both conditions are singularly alike.

The symptoms of *acute fatigue* are, first, loss of power to a greater or less extent; the irritability of the muscle being exhausted, it either refuses to respond, or responds but feebly, to the stimulus of the will. If the loss of power be total, we get practically a transient condition of true paralysis. If the loss of power be but partial, then the muscle becomes more or less "repugnant to the will," and we find that our power of adjusting the force to the act to be accomplished is lessened. Accuracy of movement and delicacy of co-ordination

are destroyed. Co-ordination may or may not depend upon the guidance of some ruling "centre," but it is certain that for the proper performance of complicated movements, each muscle requires to be educated to perform its exact proper amount of labour, so that the whole of them working together may do so in a state of harmonious concord. If one, or more, of the muscles concerned be fatigued, it fails (when required to take part in a complicated movement) to put forth its proper amount of force, and the co-ordinated movement becomes an inharmonious discord.

In certain rare instances excessive muscular exertion produces wasting of a muscle, and it is recognised as one of the occasional causes of progressive muscular atrophy.

A pianist of great eminence has informed me that after a prolonged practice he suffers from obstinacy of certain of the muscles which move the fingers. He finds himself incapable of accurately adjusting their movements, and is annoyed by an inability to strike the right note. This naturally irritates and worries him, and he finds that the mental condition thus induced only serves to make matters worse. A few hours' rest serves to restore the lost tone of the muscles, and on beginning to practise again he plays as well as ever.

Tremor of the muscles is a most common effect of acute fatigue, as everyone must have experienced.

Acute cramp-like contractions are equally commonly observed after excessive fatigue. Who has not been awoken from sleep after a long walk or a dance by painful cramp in the calves of the legs?

The pain accompanying acute fatigue must be familiar to everyone who has ever taken a long ride or has tried to hold out a weight at arm's length. This pain, be it observed, is peculiar in character and distinct from other varieties of pain.

The effects of *chronic fatigue* as observed in cases of "writer's cramp" are exactly analogous to those of acute fatigue. We find in all cases marked impairment of functional activity, but the evidence of such impairment is strangely different in different cases. In the past year I have seen ten cases of this disease. In all of them there was distinct impairment of power in one or more of the muscles of pen-prehension, or the muscles which poise the hand; but in all of the cases there was a remarkable

difference both in the muscles affected and the manner in which the weakness manifested itself. In four cases the most prominent symptom was tremor directly the attempt was made to write; in four others mere impotence to perform the act of writing was the only symptom; in one the act of writing was interrupted by the uncertain and irregular action of the muscles of pen-prehension, causing at intervals a peculiar quivering of the point of the pen; and in one other the attempt to write produced cramp-like contractions extending to many of the muscles of the fore-arm and arm. In more than half of these cases there was a feeling of fatigue, in some instances causing merely trifling annoyance, but in others amounting to pain, severe enough in one case to be described as "excruciating agony." This pain was in every case distinctly the pain of fatigue, a wearing pain more or less continuous, not following the lines of nerve-distribution, and always aggravated by the attempt to write.

The accompanying lithograph is a fac-simile of the hand-writings of eight of these patients. Numbers 1 and 2 represent impotent attempts of two patients to write their names. Having, with the greatest possible difficulty and annoyance to themselves, accomplished so much, they were utterly unable to continue, being stopped, in the one case by general spasm of the whole arm, and in the other by a refusal, as it were, of the thumb and first two fingers to hold the pen. It will be observed that the lines composing the letters are shaky and tremulous, although the pen has in each case been pressed with evident force upon the pages.

Number 3 is the writing of a lady whose loss of writing power had apparently been induced by excessive knitting. Here again the tremulous uncertainty in the formation of the letters is very evident. It will be seen that the sample of writing ends in the middle of a word, a fact due to the intense difficulty and annoyance experienced during the act. Number 4 is a somewhat similar sample; all the letters are tremulous, and become more and more illegible as the act of writing is continued.

Number 5 is a very curious and interesting specimen. Here again the letters are tremulous and uncertain in their formation

but the chief interest in this handwriting is the change which has gradually come over it. Formerly this patient wrote a bold running hand, the letters being nearly as big as in Number 6, but gradually his stroke-making power failed him, and his handwriting became more and more cramped. His stroke-making power seems to have failed because, the flexors of the thumb and fingers being used for pen-prehension instead of stroke-making, the latter movement had to be performed as best it could by the flexors and extensors of the wrist.

Number 6 is another curious sample. The letters are tolerably firm and well formed, but the act of writing was constantly interrupted by a vibratory movement of the ends of the fingers holding the pen. I have drawn vertical lines through the sample of writing at those points where such vibratory interruptions occurred. This patient's "cramp" was chiefly mental in origin. He was a highly nervous, sensitive gentleman, and sensitiveness of his skin to both faradisation and the constant current rendered it impossible to test the condition of his muscles. There was present also a neuralgic condition of the musculo-spiral nerve and its branches.

Number 7 is the handwriting of a gentleman with writer's cramp of twelve years' standing. His grasp of the pen is effected by the muscles of the fore-arm. The first dorsal interosseous muscle in this patient was most sluggish in its response to faradisation, and the intrinsic muscles of the thumb showed themselves unduly sensitive to the interrupted galvanic current. This gentleman is an amateur artist, and although he experiences but little or no difficulty in wielding the brush, the effort of drawing in pencil is very irksome to him, and the result most unsatisfactory, owing to the tremulousness of the lines. 7a represents a couple of pencil lines made by the patient.

With regard to Number 8, it might be thought that the author of this sample was fitted for a writing-master, but this is far from being the case, as his difficulty in writing is extreme. Formerly he wrote a bold running hand, but he is now reduced to a round text, which he is exceedingly slow in executing, and which demands from him an extraordinary amount of attention and great mental effort. This patient seems to have lost his muscular sense in some of the muscles

used for writing. He says that he has gradually got more and more dependent upon his eyes during the act of writing. He is obliged to watch the point of his pen very narrowly, and likes to get his eyes as nearly as possible on a level with the paper. If he looks away from his pen or attempts to write quickly, his writing at once becomes irregular and unsteady. It is quite conceivable that in fatigued muscles the muscular sense should be impaired or perverted, but this is the only patient who presented any marked symptom which might be referred to such a change.

With regard to sensibility, I have not found common sensation altered in any case, although one patient complained of a "numby" feeling in the end of the thumb. In two cases there was marked hyper-sensitiveness to the galvanic and faradic currents—most marked when the rheophores passed over any of the superficial branches of the musculo-spiral nerve. In two other cases the electric sensibility was greatly diminished over the radial half of the hand, front and back.

Of these ten patients, seven were examined with a view of ascertaining the electric irritability of the muscles of prehension, and in all of them one or more muscles were found in which the electric irritability was markedly less than on the opposite limb. Not only did the muscles require a stronger faradic current to produce contraction, but the contraction when produced was often of a jerky, undecided character.

In four cases this lessened irritability was most marked in the dorsal interossei of the first two fingers; in the fifth the short flexor was at fault; in the sixth the short flexor and adductor; and in the seventh the abductor pollicis, the first and second dorsal interosseous, and the extensor primi internodii pollicis. Of the remaining three cases, one was seen before I was alive to the necessity of closely examining these muscles; another was seen in the country, where the means of making such an examination were not at hand; and in the third the electric sensibility of the skin was too great to allow of the muscles being tested. Not only was the electric irritability of these muscles diminished, but it was found that the patients soon tired of performing muscular acts involving the use of them, such voluntary acts being far less vigorous and

decided on the diseased than on the healthy side of the body. This mode of testing muscular power by the performance of voluntary acts seems to me to be quite admissible in these cases of chronic fatigue.

The great cause of this condition of chronic fatigue is undoubtedly a *too prolonged* stimulation of the muscle, whereby its irritability becomes exhausted and its nutrition suffers.

A *too frequent* stimulation has probably the same effect, but to a less degree. The "cramp" which is described as occurring in dairymen from milking, and the occasional "cramps" of pianists, are most probably due to the *too frequent* and not the *too prolonged* stimulation of the muscles. As a laboratory experiment, we know for a fact that excess of stimulation, whether by frequent repetitions, long duration, or the too great strength of the stimulant, soon exhausts irritability; and there can be no reason why the same results should not follow the same causes during life. There can be no doubt that if the stimulation be *too severe* or *forcible*, the irritability is soon exhausted, and in some cases irremediably so. There is no difference in this respect between an artificial stimulus and the natural one of the will, and I have heard of one case, and have seen another, in which muscular impotence followed the excessive voluntary mental stimulation of a muscle. It is recorded of the late eminent composer and pianist, Mr. Schumann, that, wishing to conquer as speedily as possible the mechanical difficulties of piano-playing (which he began to study rather late in life), he was accustomed to practise gymnastic exercises with his hands. Now, the most troublesome finger to a pianist is, I am told, the ring-finger of the right hand, and to the strengthening of this finger Mr. Schumann paid particular attention. He suspended an elastic cord from the ceiling, and, passing his finger through a loop at the end, he exercised his (interossei) muscles by frequently depressing the loop. This exercise was practised so excessively, and the force required of the muscles was so great, that their irritability became apparently exhausted, and, far from strengthening the finger, the exercise rendered it worse than useless. A precisely similar accident occurred to a patient of mine who was under treatment for writer's cramp. He complained of great weak-

ness of the thumb, and among other muscles whose contractile force was lessened was the long flexor. This muscle was subjected to a series of rhythmical gymnastic exercises, and rapidly improved in power. One day, however, it occurred to me to test the strength of the muscle by opposing its action by pulling on the distal phalanx of the thumb. The patient continued to contract his muscle, evincing considerable power in doing so, and putting forth all his strength. Suddenly the power diminished: he declared that "his thumb felt as weak as ever," and the lost tone of the muscle was not recovered for three weeks or a month. We had apparently committed the same error as Schumann.

These two cases appear to me to offer instances of excessive voluntary stimulation, resulting in immediate loss of power.

In the performance of automatic acts such as writing, it would seem that the muscles are, as it were, trained to respond to the least possible amount of mental stimulation: and in proportion to the smallness of the mental stimulation required is the ease of the performance of the act and the length of time during which it can be performed continuously. Directly, however, an act which should be automatic ceases to be so (as when, for instance, one of the muscles of pen-prehension in a scrivener becomes chronically fatigued), then that act can only be accomplished by, as it were, goading on the muscles by an excessive amount of mental stimulation, and the evil which has commenced will make rapid strides if the act be persevered in, for the muscles, being over-stimulated, soon have their irritability exhausted.

Writer's cramp often has its starting-point in some piece of excessively hard work to which the patient himself refers the commencement of his troubles. Amongst the cases which have come under my own cognizance it occurred three times in professional scriveners, men who *wrote against time*; and with regard to whom it would be difficult to say that one day's work was harder than another's. In one instance it occurred in an architect who worked excessively hard to finish the ornamental details of a plan by making broad dark lines with a hard pencil, which he used with great force. In another instance it supervened in a lady who worked for two or three days against time

to finish an etching for a bazaar. Another patient, also a lady, attributed her troubles to excessive knitting.

The condition of the general health seems also to have an important influence on the disease. In one case the rapid advance was apparently due to want and anxiety of mind; and in another instance the patient dated his troubles from a severe nervous shock which he sustained owing to an alarm of fire which occurred at the Surrey Music Hall while Mr. Spurgeon was preaching there.

Although the general effect of this disease is in every case the same, yet a careful examination reveals striking differences both in the muscles affected, the behaviour of the affected muscles, and the manœuvres adopted by the patient to counter-balance his deficiencies. As far as the writer's experience has gone as yet, no two cases are alike. These differences are not surprising when one considers the striking differences which exist in the mode of writing employed by different people. It may be asked, "If the muscles of pen-prehension are really the seat of chronic fatigue, how comes it that they are useful for complicated movements other than that of writing?" My reply is, that their utility for other movements is apparent and not real. The hand and fore-arm are so bountifully supplied with muscles that for most movements we have a considerable choice of muscles; and if we find certain of them weak, we unconsciously employ others to do the required work. It is frequently noted that, although a patient is totally unable to write, he is nevertheless perfectly able to use his knife and fork, except in rare cases. Now, a moment's reflection will show that the prehension of the knife and the prehension of the pen are totally different acts; the former is effected mainly by the palm of the hand and *middle, ring, and little* fingers, while the latter is mainly effected by the *thumb* and *index* finger. My experience goes to show that acts involving the necessary use of the same muscles as are employed in writing are all performed with difficulty. The holding of a knitting-needle was impossible with one of my patients; another lost the power of holding up her dress between the thumb and first finger as she had been accustomed to do; and a third, whose supinators of the hand had become weak, had lost the

power of throwing a letter into a letter-box by means of the usual supinating movement which he was accustomed to employ. If the power of the muscles be individually tested, it will be found that those which are chronically fatigued do not contract vigorously, that the movements which they produce are more or less ineffectual, and that the muscles are quickly tired out. In extreme cases of writer's cramp, where the disease has, as it were, crept some way up the arm, it is very common to find that many movements other than that of writing are interfered with. One patient, whose case I reported last year, could neither feed himself nor dress himself without much difficulty.

There is one interesting phenomenon connected with writer's cramp which I am unable to explain, viz., the "*associated movements*" which accompany the act of writing. Readers of "*Pickwick*" will remember that when Mr. Samuel Weller took upon himself to write a valentine, he found it necessary "to recline his head on his left arm, so as to place his eyes as nearly as possible on a level with the paper, and, while glancing sideways at the letters, to form with his tongue imaginary characters to correspond." These associated movements are always an expression of weakness, and are commonly noticed in those who are unaccustomed to write, or who, like children, are learning that art. They are commonly seen too in persons with writer's cramp, who are as regards the act of writing reduced to the condition of children. One elevates the shoulder in an extraordinary fashion, another gets the head down upon the paper, and a third finds it impossible to write with one hand without making similar kinds of movement with the other. These movements seem to disappear as the power of writing returns.

Next as to the *mental condition* of these patients, which is generally markedly peculiar. I should make a distinction between the primary mental state, to which is often due, in some measure, the primary failure of the muscles, and the secondary mental state, the state of anxiety and misery which is engendered by the failure of the hand. Cases of writer's cramp vary from those which are purely physical in their origin to those which appear to be purely mental. Between the pro-

professional scrivener whose muscles are exhausted by the endless labour of "quill-driving," and the bridegroom who is unable through "nervousness" to write his name in the register, we find all degrees of mental and muscular impotence. The usual primary or original mental condition is well described by one of my patients, who says of himself: "I am one of those persons who always want to do everything twice as vigorously as is necessary, and have always been noted in the office as being over-anxious about my work. When I had writing to do, I used to do it as if it were for dear life, and especially so when pressed for time, as was often the case." This description applies more or less to all my patients. They are as a rule over-anxious persons, and such as would be likely to use an excessive amount of mental stimulation for the production of muscular contractions. They are often nervous people, such as are easily startled by sudden noises and the like. Their power of writing is usually worse in the presence of spectators, or when the matter to be written is of unusual importance, and in these respects they very closely resemble stammerers. I have met with one instance, and only one, in which writer's cramp was hereditary and had passed through three generations. The grandfather, the father, and the son had all been afflicted with occasional writer's cramp, which supervened not only after excessive writing, but whenever called upon to sign documents of importance in the presence of others. One of my patients tells me that he can write better when merely copying than when composing, as though the act of writing could be accomplished satisfactorily only when it could receive the undivided attention of the mind. For the most part these patients are very sensitive as to their condition, and it is only when the disease has made considerable progress that they can bring themselves to confide their troubles to others. They are ignorant of the existence of any such disease as writer's cramp, and they commonly entirely neglect the first symptoms of the faltering grasp of the pen, attributing it to their own "foolishness," against which they often struggle resolutely with the determination "not to be beaten." Their state is a constant source of annoyance and irritation, and thus is commenced that mental condition which I have called secondary. The mental state of

a patient with fully developed writer's cramp is one of the most distressing which can be conceived. To be constantly plagued by a right hand which lags behind the will and refuses to obey its orders, must be no light mental torment; but when the obstinate right hand is the sole source of income, and the owner of it, with all the will to work, finds every attempt to earn his living balked by no apparent cause, it is difficult to imagine any more potent source of mental agony.

One of my patients was in the most desponding state possible when first he came under my own observation, and was in fact upon the verge of suicide; and another had in a moment of fury taken a knife and stabbed his right arm at the wrist. It is needless to say that this secondary mental condition reacts upon the already damaged muscles, and that after its establishment the progress of the disease is rapid.

I have seen one case in which prolonged writing produces headache and neuralgia; and another in which a dull pain in the cervical region of the spinal cord follows prolonged efforts to write. In both cases the writing difficulty is extreme, and the worry and irritation it causes are excessive. The general health of these patients, especially when the disease has advanced, is generally below par. They are as a rule thin, their muscles are soft and flabby, and their sleeping power not good.

In five of my patients (the only five in whom I have looked to it) the condition of the finger-nails called for remark. In four of them the nails were remarkably thin and papery, very prone to break, and extremely useless for many of the purposes which nails serve, such as opening knives, &c. This condition of the nails would merely indicate that the nutrition of the limb was below par. In the fifth case, the nails were not only thin, but pitted and furrowed to a most remarkable extent. I certainly never have seen worse nails, and it has been curious to watch how, as the man's general condition and powers of writing improved, the nails improved likewise; and one could infer his writing power from an inspection of the nails.

It has been suggested that because the left hand is liable to suffer in those cases in which the patient finds himself compelled to write with it, therefore the disease is *centric* in its origin. I

cannot at all agree with this, nor do I see any necessity for such a theory. As Duchenne says, "One must always admit for the development of this malady, as for all others, a particular predisposition." Given this predisposition, with failure of the right hand already established, and with it that mental state which I have called secondary, it would be a matter of great surprise if the act which caused the right hand to fail should not *a fortiori* produce a similar condition of the left hand, which is generally the weaker of the two.

It is said that this disease is coeval with steel pens, and I should think that this is very likely the case; for a steel pen, with its hard point and rough friction over the paper, certainly demands from the muscles of pen-prehension a far greater effort to keep it steady, and the scrivener has not the opportunities offered by pen-making of resting his hand.

In the next paper I shall consider the treatment of this disease.

SOME OBSERVATIONS ON THE USE OF PHOSPHORUS IN NEURALGIA.

ILLUSTRATED WITH EIGHTEEN CASES.

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THE great difficulty in administering pure phosphorus in a reasonably palatable form, together with some doubt as to the kind of case likely to be benefited by it, and perhaps some apprehension of the effect of so powerful a poison, even in minute doses, may be among the reasons which have prevented the more frequent use of a medicine capable of sustaining comparison with the most active remedial agents with which we are acquainted.

So long ago as 1863 Dr. C. B. Radcliffe¹ published an account of four cases successfully treated with the hypophosphite of soda.² Two of them are cases of sick, or neuralgic, headache—migraine; one of trigeminal, and one of sciatic, neuralgia.

In 1868 M. Dujardin Beaumetz published the results of some investigations conducted by him in that year. These relate rather to the mode of administration than to the therapeutic action of the remedy, and accordingly will be found in brief under the former head.

Dr. Chapman,³ in his recent work, says: "Phosphorus is in my opinion more likely to intensify than to palliate neuralgia:"

¹ Radcliffe, *British Medical Journal*, Nov. 7, 1863, p. 489.

² For remarks on this substance *vide infra*.

³ Chapman, "Neuralgia and Kindred Diseases." 8vo. 1873, p. 267.

and he then quotes Dr. Anstie.¹ This author, after trial made with various preparations of phosphorus, concludes that "its utility is not very extensive or reliable;" and perhaps this opinion of the doubtful efficacy of a very disagreeable drug may also have militated against its further trial, emanating from such an authority.

In 1872 attention was re-aroused to its powers by a letter from Mr. G. M. Bradley to the *British Medical Journal* for October 26, in which he relates a cure with small doses of phosphorus, which had obstinately resisted every other reputed remedy. More recently still Dr. Broadbent² related three cures of nervous disorder treated successfully with this remedy; one only, however, of these was a case of nerve-pain.

In the middle of January of this year I put the first of the reported cases under treatment. The other seventeen followed consecutively upon it in my practice, and were noted as they arose; but they are not here set forth in that order, on account of the convenience which their classification presents in this mode of publication. The following remarks bear exclusively upon cases of nerve-pain, and these have been carefully differentiated from similar diseases. They are arranged in three classes, which are named respectively Acute Primary attacks, Acute Recurrent attacks, and Chronic cases. Of the first class there are only six cases, but it is almost characteristic of neuralgia to recur, and it will be interesting to note whether some of the rapidly-cured primary cases are liable to anything like an early return of the disease. The third division of six chronic cases do not present such perfect results as the two former classes; still, if it be remembered in considering them that two were complicated with phthisis—one being advanced in that disease—while a third was already reduced to a dangerous condition of debility before coming under treatment, I think it will be readily allowed that the result is on the whole eminently satisfactory. In the Tables setting out these cases the conditions and events of them are classified under headings which speak for themselves. Under "Complication," if debility or anæmia be inserted, it has been of a very marked kind.

¹ Anstie, "Neuralgia." London, 1871, p. 180.

² Broadbent, *Practitioner*, April 1873.

Occasionally the time occupied by the cure seems disproportionate to the duration of the disease; but I would now observe, what will be found repeated hereafter, that in all cases ultimately benefited relief followed the first few doses, although the cure could not fairly be considered complete until all soreness or numbness—all symptoms of disturbed sensation—had been removed. The figures and letters, F. 1, F. 2, refer to the formulæ which are given below in that part of these observations which bears upon the best mode of administering the remedy, and the experience gained in the trial of various prescriptions. If the remarks upon these points appear rather long, I trust that the fact that they are for the most part original, and, as far as I have been able to ascertain, now for the first time collected and published together, will be a sufficient excuse. Only those who have tried to administer the oily mixture know the disgust with which it is regarded, and the impossibility of persuading patients to persevere with it. On this, for instance, the watery solution is a great advance; and I am not aware of any hitherto published directions for its use.

Dose of Phosphorus.—The dose of pure phosphorus is stated in the British Pharmacopœia to range from $\frac{1}{30}$ to $\frac{1}{10}$ of a grain. Dr. Radcliffe and Dr. Broadbent (loc. cit.) have used the former dose with success; Dr. Anstie with little or none. Dr. Adolphe Waltuch,¹ in his "Dictionary of Materia Medica and Therapeutics," gives the dose as being from $\frac{1}{20}$ to $\frac{1}{4}$ of a grain.

The last quotation alone gives the proper range of dose of this remedy. To prescribe less than $\frac{1}{30}$ of a grain in the first place is to render its therapeutic action apparently variable or uncertain. In some of my earlier cases I am of opinion that recovery was unduly delayed in consequence of the small quantity with which treatment was begun; at least it will be observed that alleviation of the symptoms commenced much sooner in those cases last under my care. I now invariably begin by giving $\frac{1}{12}$ of a grain every four hours, and this I conclude to be an average dose; it has proved itself a perfectly safe and very efficacious one. In only one case will it be observed that any serious symptoms arose which might perhaps

¹ A. Waltuch, "Dictionary of Materia Medica and Therapeutics," 1868, p. 247.

be attributed to the remedy, and in this instance $\frac{1}{12}$ of a grain was the quantity prescribed. The symptoms appeared, however, to be in a measure critical, and, since they have on no occasion repeated themselves, may have rather been induced than originated by the medicine employed. On the other hand, Case No. 17 arrived at taking by accident $\frac{1}{4}$ of a grain every four hours, and continued to do so for some days, without harm. On consideration, I conclude that to be on the safe side; and at the same time, to secure the proper therapeutic action of the drug, treatment may commence with $\frac{1}{18}$ of a grain repeated every four hours, which should be continued for six doses. If there be now only slight or no alleviation of the symptoms, the dose should be increased to $\frac{1}{12}$ of a grain, and continued at similar intervals. After the latter quantity has been taken for forty-eight hours, I am inclined to think that some good may be effected—i.e. if cure have not already resulted—by increasing the dose still further, either in quantity or frequency of administration; but I do not think that any good will be effected by a larger dose of which $\frac{1}{12}$ of a grain has not given decided promise. I have not, however, had a sufficient opportunity of investigating the action of larger doses.

Of the Mode of Administration.—The mode of administering pure phosphorus is of the first consideration, for two reasons: the ease with which it alters under exposure to certain bodies, and the extreme nauseousness of the fluid preparations.¹

Phosphorus may be dispensed from its solutions in oil, in ether, in chloroform, in spirit; or it may be given enclosed in capsules or pills (Radcliffe, Broadbent); or in loose chemical combination, as in the hypophosphite of soda (Radcliffe, Anstie), or phosphide of zinc. In the course of the experiments alluded to above, conducted in 1868, M. Dujardin Beaumetz² was led to reject the solutions in ether and chloroform, on account of the

¹ Both of these difficulties, I believe, I have succeeded in overcoming. A remark of Dr. Radcliffe's, however—to the effect that the disappearance of phosphoric odour from a solution is presumptive evidence of oxidation of the phosphorus—deters me from stating here the formula for an almost tasteless solution. I hope to be permitted an early opportunity of publishing ten or more additional cases (some of them already successfully treated) in which I am testing this prescription.

² Beaumetz, *Gazette des Hôpitaux*, May 30th, 1868; *Journal de Pharm. et Chimie*, 1868, p. 227.

deleterious effect of the solvent in repeated doses. Spirit he rejected on account of the small quantity dissolved by it—one part in 320, at a specific gravity of .799. Nevertheless, this is the preparation which I have chiefly used, and I find that with the aid of heat ten grains of phosphorus dropped into 1,000 drops of absolute alcohol do disappear, and are not again precipitated. M. Beaumetz selected finally oil of sweet almonds as the best solvent. This preparation must either be made into an emulsion, in which form it is extremely nauseous, or it may be enclosed in capsules. These were found, however, to deposit after a time an insoluble, and therefore presumably inactive, form of phosphorus. The means of avoiding this is to employ oil which has been superheated, which will be found to make a clear and stable solution. No doubt if the capsules and pills now obtainable are guaranteed to be made with oil thus prepared, they afford an agreeable and perhaps sufficiently efficient means of administration; but I believe it is conceded that there are often advantages enjoyed by fluid forms of medicine which are either largely diluted in the first place, or which admit of easy and rapid solution in the fluids of the body. The pills as recommended by Dr. Radcliffe are made by heating phosphorus and suet together in a close vessel, in the proportion of one part to 100, making into three-grain pills, and coating with gelatine. The oily solution is a preparation of the French codex (phosphorus 2, oil of sweet almonds 100; dissolve with heat), and of the Prussian (phosphorus 1, oil of sweet almonds 80; dissolve with heat). A French formula for emulsion (Soubeiran) is, phosphorised oil 8, gum arabic 8, peppermint water 100, syrup 60; make an emulsion.

The four successive cases related by Dr. Radcliffe (*loc. cit.*) were treated with the hypophosphite of soda in doses of from seven to ten grains three times a day; and Dr. Anstie has used the same quantity of this remedy in similar cases of nerve-pain, in which he did not find it successful, although he was satisfied with its effects in cases of chronic alcoholism.¹ It is said to be not unpleasant to take. Phosphide of zinc contains one-fourth of its weight of pure phosphorus; but two parts of the compound must be administered in order to produce the effect of one

¹ Reynolds, *System of Medicine*, art. "Alcoholism"

part of phosphorus. I have no experience of these forms. Dr. Anstie has used pills containing $\frac{1}{4}$ of a grain of phosphorus; these were made according to Dr. Radcliffe's formula; he was dissatisfied with the result. In two cases I used coated pills containing the same quantity, and made by Messrs. Kirby and Co.; they appeared to be without effect, although I gave twice the dose administered by Dr. Anstie, and was afterwards successful with the remedy in other forms. Dr. Broadbent used capsules in his three cases of nervous disorder with good effect.

The discrepancy may now be noted between the results obtained by two competent observers with the hypophosphite of soda used in similar cases. Is this an unstable compound? Again, the results obtained from capsules are opposed to those obtained with pills—which, I believe, are of the same or similar composition. Can any such deposit of insoluble phosphorus have occurred in the pills, such as is indicated by M. Beaumetz?

I have used the oil (with the action of which also Dr. Anstie was not pleased) in five cases—exclusively, in three. The formula employed was—

FORMULA No. 1.

¹ Phosphorised oil . . .	3 drachms.
Powdered gum arabic . .	6 "
Spirits of peppermint . .	$\frac{1}{2}$ drachm.
Water to six ounces.	

I have avoided the use of syrup (as recommended by Soubeiran), or of chloric ether, both of which increase the nauseousness of the mixture. As tincture of orange is the best vehicle for chloral, so spirit of peppermint is incomparably the best vehicle for phosphorus. This mixture keeps reasonably well, and is, I am inclined to think, a very active preparation—perhaps more active than any other. It is, however, so extremely nasty, and moreover seems of itself to disagree so

¹ This preparation contains one grain of phosphorus to three drachms of oil, which seems to be the smallest proportion which can conveniently be used for dispensing.

with the stomach, that I have not more than one patient who professed to have retained more than fourteen doses.

Tincture of phosphorus should be made by dissolving the material in absolute alcohol with the assistance of heat. The most convenient proportion for dispensing is: phosphorus, one grain; absolute alcohol, three drachms. This mixture will, I know, retain its powers for six weeks, although not kept in a full bottle; I do not know that it loses its efficacy after this period. Before adding this tincture to water, it is necessary to dilute it still further with rectified spirit; otherwise hydrated phosphorus will be precipitated. The formula I have employed is the following:—

FORMULA No. 2.

Tincture of phosphorus	3 drachms.
Rectified spirit	2 „
Spirits of peppermint	$\frac{1}{2}$ drachm.
Water to six ounces.	

This forms an active, efficient, and convenient preparation. It is not quite disgusting, and will be readily taken and well tolerated by those who have begun to experience benefit from the first few doses. It is unstable, and should therefore be supplied to the patient in quantities not calculated to last more than twenty-four hours. I have used this mixture in the majority of cases.

(The table of cases will be found on the next page.)

Observations.—The first general observation which presents itself is, that chronic cases are cured the least rapidly when they are cured at all; while among them is found the only absolute failure of the remedy employed. This obstinacy may be referred to one of the following causes:—First, the habit of pain into which the nerves have got, and the reaction upon their functional activity which the debilitating effect of constant pain must have; or, secondly, the extreme or particular kind of debility to which the complicating disease has reduced the subject; or progress of a specific nerve disease beyond the possibility of rapid recovery; or, lastly, the necessity for time to recover from the condition of anæmia which in a particular

No.	Sex.	Age.	Nerves affected	Duration of attack.	Extreme duration of treatment.	Complication.	Result.	Dose and form of remedy.
PRIMARY ACUTE CASES.								
1	M.	40	R. Trigeminal	4 days	4 days	Catarrh	Recovery	F. 2, gr. $\frac{1}{2}$ 4tia. h.
2	F.	26	L. "	14 "	10 "	Anæmia	"	F. 2, gr. $\frac{1}{2}$ 2dis. h.
3	F.	25	"	21 "	24 hours	None	"	F. 2, gr. $\frac{1}{2}$ 2dis. h.
4	M.	46	Cervico-occipital	12 hours	12 "	General derangement	"	F. 2, gr. $\frac{1}{2}$ 2dis. h.
5	F.	28	L. Trigeminal	14 days	48 "	Lactation	"	F. 2, gr. $\frac{1}{2}$ 2dis. h.
6	F.	26	"	6 "	12 days	Catarrh	"	F. 1 and 2, gr. $\frac{1}{2}$ om. hora, $\frac{1}{2}$ 3tia.
RECURRENT ACUTE CASES.								
7	F.	60	R. Sciatic	15 days	36 hours	Decay of nature	Recovery	F. 1, gr. $\frac{1}{2}$ 4tia.
8	F.	33	L. Trigeminal	5 "	6 days	None	"	F. 1, gr. $\frac{1}{2}$ om. h. F. 1, gr. $\frac{1}{2}$ 3tia.
9	F.	32	"	21 "	24 hours	"	"	F. 2, gr. $\frac{1}{2}$ 8via.
10	F.	35	R. "	10 "	4 days	Lactation	"	F. 2, gr. $\frac{1}{2}$ 4tis.
11	F.	30	"	14 "	5 days	Phthisis	"	F. 2, gr. $\frac{1}{2}$ — 4tia.
12	F.	30	"	7 "	48 hours	Debility	"	F. 2, gr. $\frac{1}{2}$ 2dis.
CHRONIC CASES.								
13	F.	28	R. and L. Trigeminal	18 months	5 weeks	Phthisis	Relief	F. 1 and 2, gr. $\frac{1}{2}$ — 4tia.
14	F.	24	Cervico-brachial	4 weeks	9 days	"	"	F. 1, gr. $\frac{1}{2}$ 4tis.
15	M.	35	R. and L. Trigeminal	12 months	12 "	Nervous debility	Cure	F. 2, gr. $\frac{1}{2}$ — 4tis.
16	F.	36	Occipital, R. and L. Trigeminal	2 "	14 "	Pregnancy	"	F. 2, gr. $\frac{1}{2}$ — 4tis.
17	F.	26	Cervico-brachial	16 years	18 "	None	"	F. 2, gr. $\frac{1}{2}$ — 4tis.
18	F.	40	R. and L. Trigeminal	4 months	15 "	" (decayed teeth) ?	None	F. 1 and 2, gr. $\frac{1}{2}$ 4tis.

case may be the prime cause of the symptoms. For example, Case No. 13 was complicated with phthisis, which was advancing slowly but distinctly, and in addition was the true subject of a great deal of depressing mental irritation. Almost constantly for the last eighteen months she had been subject to nerve-pain affecting the cervico-occipital and brachial nerves, and, on bad days, both trigeminal nerves. She was under observation altogether for about three months, being treated in the first place with every kind of remedy, including muriate of ammonia. In my experience this remedy, administered in doses of from one to two drachms every two or three hours, ranks next to phosphorus in efficacy; but it must be given in doses as large and frequent as these; and in those cases which require the remedy—in neuralgia—I do not remember ever to have found it productive of sickness. Finally, since the attacks recurred with added severity every third day, she took large doses of quinine without effect. She was now put upon $\frac{1}{2}$ of a grain of phosphorus dissolved in oil, and she derived some relief from the nine doses which she was able to retain; after that the stomach rejected the mixture. She then took $\frac{1}{2}$ of a grain every four hours in F. No. 2 for about five weeks, when she removed to the country. She was considerably relieved, but not by any means cured.

Case 14 was also complicated with phthisis in an early stage. She too began by taking F. No. 1, but was obliged to relinquish it. She took F. No. 2 ($\frac{1}{2}$ of a grain) for other seven days, and was relieved, but not cured. She, too, now removed into the country. It is worthy of remark that neither of these cases experienced the least improvement in general health, and no alleviation of the phthisical symptoms. I believe that it was Dr. Churchill, of Paris, who so strongly advocated phosphorus in consumption.

Case 18 had a very large number of decayed teeth, and it is a question whether she was not suffering from simple tooth-ache; a kind of pain which those patients who suffer both from it and from neuralgia describe in very different—in distinct—terms. She had suffered four months when she came under treatment, and was already reduced to a condition of very dangerous debility. She took both formulæ without the least benefit.

Against these three cases we may put Nos. 15, 16, and 17. No. 17 is a remarkable example of the power of the remedy over a favourable subject. This lady had suffered from cranial neuralgia for sixteen years, and during the whole of that period could not recall one entire week of freedom from pain. Her father had suffered continuously for many years. Her catamenial periods, her marriage, her pregnancies, made no difference to her in this respect. She took F. No. 2 ($\frac{1}{4}$ of a grain) every four hours, and subsequently every three hours. After six doses she became entirely free from pain, and continued so for eight days. She then experienced a slight return of it, and she increased her dose—without advice, of course—to $\frac{1}{2}$ of a grain, which she took for three days without harm. She soon entirely recovered.

There is one other remark to be made concerning this group of six cases—the concurrence of right and left trigeminal pain in four of them. I was unable to obtain satisfactory information as to the side first attacked.

In the other two classes of primary acute and recurrent acute cases there is a curious equality in the numbers cured in a remarkably short time, as against those in which the process occupied a longer period. In each class are three cases in which the average duration of treatment was respectively 28 and 32 hours. In these cases the period of illness does not seem to bear any proportion to the period of recovery. For example, Cases 3, 5, 9, and 12 were ill respectively 21, 14, 21, and 7 days; yet they recovered in 24, 48, 24, and 48 hours—a far shorter period in proportion than that in which the remaining cases recovered.

Case No. 7—one of sciatic pain occurring at that time of life known as the period of decay—was the first I treated with phosphorus. Six years previously she had recovered from an attack which had lasted her for two years with scarcely any intermission. She was a poor woman, of spare habit of body, sixty years of age, and enfeebled by privation and trouble. She took F. No. 1 ($\frac{1}{4}$ of a grain), every four hours, for six doses. Most severe vomiting and purging now made their appearance, and in visiting her I was seriously apprehensive that death would shortly ensue. She had lost all pain—all

neuralgic pain—however; and she ultimately recovered. She had no relapse. I could not quite make out whether the pain disappeared before or after the purging; and of course the question arises whether this was not a case of sciatic pain from fæcal accumulation, which croton oil would have relieved with equal promptitude. I do not think so, however; and, further, I doubt whether the serious symptoms were owing to the remedies used. They may have been critical; and, in any case, this is a doubtful example of the use of phosphorus.

I remarked above that the time occupied by treatment, as marked in the sixth column of the table of cases, shows the extreme space occupied in the *cure*; but most of these cases experienced very remarkable relief in a few doses. This the following table will make plain :—

Case No.	1	obtained great relief in	.	.	.	7 doses.
"	2	"	"	.	.	3 "
"	4	"	"	.	.	2 "
"	6	"	"	.	.	8 "
"	9	"	"	.	.	5 "
"	13	"	"	.	.	10 "
"	14	"	"	.	.	2 "
"	17	"	"	.	.	4 "

I close the present series of cases with these few remarks, which, with the table of cases, will enable the reader to draw his own conclusions as to the value of this plan of treatment. Nothing has here been said of hygienic measures; but these, which, both as remedial and as prophylactic, are indispensable to a successful treatment, have been already set forth in the most complete way by various authors upon this subject, and do not therefore need repetition in these observations upon the use of a somewhat neglected but very useful remedy.

NOTE ON THE PARALYSING ACTION OF ACONITE ON THE SYMPATHETIC NERVE.

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Assistant Physician to the East Sussex and Hastings Infirmary.

THE topical action of aconite was displayed in the following case in so marked a manner as to be worth recording.

A young lady, aged nineteen, came under my care in November 1872, who had long been suffering from spinal irritation and neuralgia. During the previous summer she had passed some time at St. Moritz with benefit, and could then walk four or five miles. A recurrence of severe pain, sometimes in the spine, and sometimes in the face, head, and neck, had induced her to seek further change at Hastings. On the 3rd of November, having recovered from an attack of spinal pain, the patient took a short walk while a north-east wind was blowing, and came home with severe pains in the head and neck, chiefly on the left side. The effect of the subcutaneous injection of $\frac{1}{4}$ grain of morphia was to induce sickness, while it failed to relieve pain or to procure sleep. Liniment of chloroform and belladonna was applied, but she passed a disturbed night, and next day was suffering much pain along the course of the left occipito-cervical and trigeminal nerves. I applied the liniment of aconite (B. P.) behind the left ear and down the neck, carefully avoiding a small place which had been blistered by the chloroform liniment. Some relief was obtained, and the liniment was again painted on twice that day (5th Nov.), and produced some numbness and tingling of the lips. On the morning of the 6th more liniment was applied, as this was found to be "the only thing that drove away the pain." At 9 A.M. the eyes and nose felt sore and tingling "as if she had a cold in the head;" at 1 P.M. burning

soreness of the eyes was increasing, the skin of the neck was numb, and the lips tingled much; at 3 P.M. she was screening her eyes from the light, but rejoicing in being free from the neuralgia. The left eyelid was much swollen. The vessels of the ocular conjunctiva were enlarged, bulging, and tortuous, and of a bright pink hue. The forehead, face, ear, and neck were hot to the touch, while the right side was cool. The pupil was widely dilated, and vision was greatly impaired, so that she was unable to read good letterpress, but could read large print of a title-page.

The lips were dry. The tongue was projected somewhat to the affected side. This was tried twice. There were no general symptoms, no weakness. The pulse was 88. At 6 P.M. pain in the eyes and intolerance of light were still greater, but she was free from neuralgia and perfectly well, the eyes excepted. The pupil being widely dilated, I examined with the ophthalmoscope, and saw the *arteria centralis* distended and the retina generally congested. Sensibility, tested by pricking, was greatly impaired over the whole of the side of the neck and face to which the aconite had been applied.

The symptoms abated at 9 P.M., and next morning (11 A.M.) the congestion of the eye had almost subsided, slight redness and swelling only remaining. The left pupil was still larger than the right; both contracted well to the stimulus of light. There was no return of neuralgia.

It is not uncommon to meet with less marked cases of the effects produced by the topical use of aconite, thus:—

Mr. W. S., aged 50, suffering from facial neuralgia, rubbed aconite on the cheek-bone. It caused numbness and “twittering” sensation of the part, which lasted some hours, while the pain was dulled. There was sensible increase of warmth of the muscles of the face. The eye became weak and watering, while the lid drooped. The state of the pupil was not noted. This condition lasted about twenty-four hours.

Mrs. W. D., aged 57, used aconite liniment with temporary relief to her neuralgia, and with increased warmth to the facial muscles and manifest congestion of the conjunctival vessels.

These and similar cases point to the fact that aconite applied to the skin in the neighbourhood of the cervical ganglia and

branches of the sympathetic, produces consequences similar to those brought about by section of the nerve in that situation in animals. In both cases a swollen and congested state of the conjunctival vessels and abnormal warmth of the part is the result.

In both the threefold office ascribed to the sympathetic is interfered with: (1) sensation is blunted in the parts supplied by the nerve; (2) nervous control over the blood-vessels is withdrawn; (3) motive power is reduced.

Aconite thus becomes a powerful local remedy in our hands, not only as a controller of morbid sensation, but as a retarder of vascular action, and by consequence as a modifier of muscular action.

But although generally a painless remedy, is it a safe and innocuous one? Happily, it appears that the small quantities of the drug capable of being absorbed through the skin, attack the nerves and muscles directly in their neighbourhood, and show little disposition to be absorbed into the general current of the circulation, and so paralyse the great central circulating organ. In the above detailed case the local action of the remedy was powerfully exerted, but neither stomach, kidneys, nor heart seemed to sympathise. None the less, however, is caution needed in the use of so powerful an agent, lest its action should pass on to the cardiac nerves and vessels, and so produce grave symptoms.

The following case of general aconite poisoning illustrates some of these symptoms, and shows at the same time how large an internal dose of aconite may be recovered from. In December 1871 Mrs. G. was attending the Hastings Infirmary as out-patient, suffering from pain in the shoulder, and was ordered a liniment composed of equal parts lin. opii and lin. aconiti (B. P.): she was duly cautioned as to the poisonous nature of the application. On January 5 she drank off about six drachms of the liniment, *i.e.* three drachms of lin. aconiti. Immediately afterwards she felt a burning sensation in the throat and tongue; in about ten minutes she was unable to speak, and is said to have made a stertorous noise in breathing; she suffered from a strangling sensation in the throat. The face was partly pallid, partly flushed (over the cheek-bone). A neighbouring chemist who was called in encouraged vomiting, which had

already set in, with mustard emetics, followed by ʒss of sulphate of zinc: he did not yet know what poison had been taken. I saw the patient an hour and a half after the dose had been taken; she was then agitated and weak, with a pulse of 84 and a cool skin; her only complaint was of burning at the throat and stomach, and weakness. *The pupils were larger than natural.* She took some brandy and raw egg, and a stimulating draught from time to time. Next day she felt faint and low, and some amount of burning sensation in the throat and stomach continued; the face was flushed, the pulse 80 and good; the urine remained normal, and much milk was secreted. On the 7th she appeared to be well.

In cases of fatal aconite poisoning the symptoms seem to have followed this order:—

1. Tingling and numbness of the tongue and throat, soon extending to the stomach, with local sensations of heat.
2. Failure of the circulation, evidenced by cold extremities, cold sweats, great feebleness, &c.
3. Pupils dilated and sight dimmed.
4. Fatal syncope.

[As a pendant to Dr. Bagshawe's cases of the local action of aconite upon the vaso-motor nerves, I beg to mention the following, which occurred some months ago in the out-patient room:—E. M., a small and thin woman, aged 29, married, but has never had children: she had had threatenings of phthisis, and had also suffered intercostal neuralgia. Just after Dr. Bagshawe had informed me, verbally, of his first case, this woman applied one day at the hospital, suffering from severe pain in the left trigeminus (first and second division), which was evidently excited by a very bad carious tooth. She would not have the tooth extracted, and, as an experiment upon the powers of aconite, I ordered the *lin. aconiti* to be painted over the painful parts. The effect in arresting pain was immediate and complete, but on the next hospital day she came complaining of "erysipelas." The parts which had been painted were swollen and bright red; moreover the conjunctiva of that side was severely congested, with *tortuous vessels*. The pupil in this case was contracted rather than dilated; and this leads me to notice

the curious fact that in Dr. Bagshawe's cases the pupils were dilated, in his first case to a remarkable extent. I need not say that, while the rest of the phenomena agree well enough with the idea of sympathetic paralysis, dilatation of the pupil is exactly the reverse of the condition of iris produced in Bernard's and Brown-Séquard's experiments.—F. E. ANSTIE.]

ON VESICATING INSECTS.

BY DR. FUMOUGE, PARIS.

VESICATING insects live in all the countries of the globe, and more especially the hot and temperate climates ; and there is perhaps not a people which is not acquainted with their properties and knows not how to utilise them. In Europe, one kind is more particularly employed ; namely, officinal cantharides. It is pre-eminently *the* vesicating insect, or at least the one which holds the first rank in European pharmacopœias. And yet it would be a mistake to conclude, from its place in the *Materia Medica*, that it possesses vesicating properties in the highest degree : it will be seen further that it is not so. Its vesicating properties are certainly quite sufficient for the wants of therapeutics, and this explains how its use has become so prevalent ; but if it is so employed in preference to other more vesicating insects, it is most especially because it is more easily gathered. Cantharides, as is well known, alight in swarms upon certain trees ; it is therefore possible to collect thousands of them in a few instants, whilst this is not the case with other vesicating insects. Sometimes, as with the meloës, it is even necessary to catch them one by one, and it follows that, being thus gathered, they are far more expensive than cantharides.

Such is, I believe, the cause of its almost exclusive, and I think I may say its quite exclusive, employment in pharmaceutical laboratories, as it has superseded the use of meloë and mylabris even in the countries where these were formerly employed ; so that I am surprised to read, in the most recent treatises of *Materia Medica*, that the *Mylabris* and *Meloë majalis*

or *Proscarabæus* are employed, the former in Italy and the East, the latter in Spain. Already, several years ago, as I desired to study these insects, I asked for samples of them in Italy, Turkey, and Spain, on the strength of authors' statements; and the answer invariably was, that they no longer existed in commerce. It was with the greatest difficulty that I was enabled to procure a few pounds of Meloë.

There is, however, one mylabris, the *Mylabris Sidæ*, better known commercially by the name of China Mylabris, and which for several years has reached us from China in pretty large quantities, and may be procured in the London Docks. This insect affords on analysis as much cantharidine as cantharides; but as it is less abundant in fatty matter, it is not, I think, as suitable as cantharides for the preparation of vesicating plasters.

The richness in cantharidine of vesicating insects is certainly very variable, and it would be very interesting to classify them according to their degree of strength, as it would be easy to do if the chemists who have had occasion to analyse them had not generally contented themselves with asserting that such and such a kind was vesicating, without stating the richness in cantharidine. This blank in the history of vesicating insects, though it is to be regretted, is, however, of slight importance for the pharmaceutical chemist, who habitually in commerce only meets with the officinal cantharides, occasionally the China Mylabris, and scarcely ever the Meloë. These are, therefore, the only vesicating insects about which he cares to be informed, as regards their richness in cantharidine.

The *Meloë majalis* is perhaps, of all sorts, the one which contains the most cantharidine. According to my analyses, it contains 12 parts to 1,000. It is, therefore, not allowable to substitute it for cantharides in pharmaceutical preparations, as this latter insect only affords 4 parts of cantharides to 1,000, or only 5 at the maximum.

The China Mylabris affords, as I have said, as much cantharidine as cantharides do, but for reasons above stated it could not be used with advantage instead of cantharides in the preparation of vesicating plasters.

The pharmaceutical chemist should, therefore, not think of

using Meloë or Mylabris for his preparations, and should only employ cantharides, and even that with caution, as all cantharides do not contain the same quantity of cantharidine, and may be adulterated, as happens more often than is suspected. In order to make out their proportion of active principle and not be the dupe of cheats, he has only the resource of analysis.

Researches of this kind must be made with care and promptitude, and both these conditions are, I think, fulfilled by a method of analysis which M. Mortheux and myself made known some years ago.

Though this procedure has already been described in a former publication of mine, it may be of use to give an account of it here. First, we put into a displacing apparatus 100 or even 50 grammes of the cantharides, whose strength we wish to determine, and which have been previously triturated in a mortar; then we pour on the cantharides a sufficient quantity of chloroform to immerse them entirely; and after twenty-five hours the cock of the apparatus is opened so as to let the tincture of cantharides run out. In this way the cantharides are subjected to three successive macerations, and the tincture afforded by the three macerations is distilled in the *bain-marie*, so as to give out all its chloroform. The extract thus obtained is mixed with an equal quantity of sulphide of carbon, and then the liquid is agitated and poured on a filter. The filter retains the cantharidine, which must be washed with a little sulphide of carbon. When thus obtained it is not entirely pure, but it is sufficiently so to enable us to ascertain the therapeutical action and commercial value of cantharides.

By analysing with this procedure various specimens of cantharides, I have been able to find out that the French Codex has adopted too high a figure in fixing at 5 per cent. the minimum quantity of cantharidine contained in cantharides of good quality. It ought to have fixed upon 4 per cent., and perhaps even upon 3 per cent., as cantharides containing only this proportion have appeared to me to be quite strong enough.

I am also surprised to see that the commission of the French Codex and authors of the British Pharmacopœia do not recommend to prepare tincture of cantharides with such a quantity

of cantharides as to cause this preparation to present invariably the same proportion of cantharidine.

If the following table of mine be consulted, it will be granted that the justness of my remark is obvious on noticing that the proportion of cantharidine in cantharides may vary from 1·75 to 5 per cent. ; and I have no need to insist on pointing out how untrustworthy a medicament is, prepared with a fixed quantity of a substance the active principle of which may vary in such a degree. This is a real danger when, as with tincture of cantharides, the preparation may be prescribed internally.

Origin of Cantharides.	Quantity used.	Cantharidine obtained.	Cantharidine contained in 1000 grm.
	Grammes.	Grm.	Grm.
1. Cantharides from France, 1866 . .	100	0·48	4·80
2. " " " 1866 . .	"	0·275	2·75
3. " " Germany, 1866 . .	"	0·435	4·35
4. " " Trieste, 1866 . .	"	0·50	5·00
5. " " France, 1865 . .	"	0·375	3·75
6. " " Germany, 1865 . .	"	0·215	2·15
7. " " " 1865 . .	"	0·17	1·70
8. " " " 1866 . .	"	0·48	4·80

The British Pharmacopœia ought also, I think, to have indicated the minimum quantity of active principle which cantharides, employed for the preparation of blisters or epispastic papers, ought to contain. It is obvious that, if need be, cantharides containing more or less cantharidine may be used for those preparations, but only within certain limits which should not be exceeded. The epispastic papers and blistering tissues which for years have been prepared in France according to my method and by the name of Albespeyres contain as nearly as possible 4 per cent. of cantharidine, and I think that this constitutes a valuable property and a real improvement in them.

Before concluding this paper it may be good to notice the trials which have lately been made with the object of replacing cantharides in the preparation of blistering plasters by cantharidine and cantharidate of potash.

The vesicating tissues prepared with cantharidine have until now been very unsatisfactory. The surface easily breaks, and

they soon lose their properties on account of the slow evaporation of the cantharidine, which as is well known, gives off vapours at the ordinary temperature. In order to prevent this last inconvenience, trials have been made of cantharidate of potash, which presents greater stability. Experience will show whether vesicating tissues prepared with the cantharidate are brittle or not; meanwhile it is quite obvious that if tissues thus prepared do not blister more quickly than those made with cantharides, there can be no advantage in using the cantharidate, which is so much dearer.

Vesicating tissues made with cantharides are at present the best agent we possess for producing vesication.

Reviews.

Grundsätze bei Behandlung der Syphilis. Von DR. BERNHARD BRANDIS, Arzt in Aachen. Berlin: Hirschwald. 1870.

THIS pamphlet, which escaped our notice at the time of its publication, deals with questions which happen to have been recently brought under our personal notice in a striking manner; and as no time can be inappropriate for the discussion of a topic of such constant importance in practice as the treatment of constitutional syphilis, we hope to be excused for commenting upon it at some length.

It would probably be impossible to find, even in the notoriously debateable territory of therapeutics, a question which has excited such disputes and evoked such point-blank contradiction of opinions as that of the use of mercury in syphilis. The fluctuations and diversities of judgment with regard to it have at no time been more remarkable than during the last thirty years; and even now, though the weight of professional opinion is leaning strongly towards the affirmative view, there are persons of intelligence, and even of considerable experience, who denounce the anti-syphilitic employment of mercury in unmeasured terms, declaring it to be both useless and mischievous. To our mind, this last fact affords a very striking proof of the small amount of preliminary mental discipline with which men too commonly undertake the investigation of therapeutic questions. The scepticism that was pardonable and even very salutary as a rebound from the indiscriminate faith in mercurialisation, which formerly did such an immensity of mischief, is now wholly without excuse. Everyone now admits (in theory at least) that the production of pyæmia is needless and wrong; but the very authorities who have most strenuously urged this view have adduced most copious and varied proofs of the beneficial action of small doses of mercury. On the whole it seems evident that the wholesale opponents of mercury in syphilis must have altogether failed to follow up their cases, or they would certainly have encountered numerous instances of

the recurrence of the constitutional disease in the most violent and uncontrollable forms in patients of whose "cure" they had complacently assured themselves. And, on the other hand, it appears impossible that they can have paid any attention to large special classes of syphilitic affections—*e.g.*, infantile and visceral—or the superiority of mercurial treatment, for the majority of such cases must have forced itself upon their notice. To all such unbelievers we should recommend the perusal of Dr. Brandis's pamphlet; and if they feel inclined (as we also do) to discount some of the author's opinions on the score of a too enthusiastic bias which makes itself evident here and there, they will nevertheless scarcely fail to perceive that his evidence immensely strengthens the position of mercury as a curative agent in constitutional syphilis. Dr. Brandis calls mercury the "Gegen-gift" (counter-poison) of syphilis; and the use of this word, whether perfectly accurate or not, by so acute and experienced an observer, speaks strongly for the positive character of the facts which have come under his notice.

To those who have never visited Aix-la-Chapelle, it may be necessary to say a few words as to the character of the treatment employed there. The ancient reputation of the sulphurous and alkaline waters is well known, but the special use of mercury (as a supplement to the waters, in syphilis) by Dr. Brandis is probably not very generally understood by English medical men. The mode of application which he uniformly adopts is exclusively that of inunction, and it will certainly surprise a good many persons to learn that an average of thirty-three consecutive daily frictions are administered, 60 to 80 grains (for an adult) or 24 to 32 grains (for a child) of mercurial ointment being used in the course of each day; and sometimes a far longer course of continuous frictions is employed. One would naturally expect that pytalism must be a not infrequent result of this treatment; but Dr. Brandis expressly states that salivation ought always to be avoided. We shall have to refer to this subject again presently; meanwhile we may quote at length two very interesting examples of the benefits from prolonged mercurial treatment in Dr. Brandis's hands:—

1. A boy, four years old, came under treatment in the early part of 1866. His father and mother had suffered long from syphilis, and the former was dead of consumption. The boy was badly nourished, and had from infancy suffered from diarrhoea and bronchitis; it had often seemed doubtful if he could be reared. An inflammation of the cornea was now added to his other afflictions: the eyes streamed with tears, and the child buried its head deep in the clothes in the darkest room. The boy was immediately submitted to the inunction treatment: he had a lukewarm bath daily, after which half a drachm of mer-

curial ointment was rubbed in at two places, while the gums were carefully rinsed with tincture of galls; mydriasis was steadily maintained by means of atropine. As the opacity of the cornea continuously increased during the first weeks, Dr. Brandis employed, in addition, iodide of potassium, by the stomach, 10 grains daily. At the same time he paid attention to the ventilation of the room, and fed the child with milk and concentrated meat-soup. Under this treatment, while not a trace of salivation appeared, nor any other unpleasant consequences, the boy was kept for eight weeks before he could even open his eyes; but from this point there was good progress, and in the course of the following year both corneæ were completely cleared of opacity. It was remarkable that the boy increased in general strength after the treatment; at the date of writing (1870) he was attending school, and had ceased to require any special care.

The other child was treated in 1867. His father had been under Brandis some years before, with perforating ulcer of the soft palate, and was cured by a six weeks' course of inunction: the cure remained permanent. This child's case was almost exactly similar in its course to that just related; but on the one hand this boy became much stronger after treatment than the other; and, on the other hand, the last traces of opacity much more gradually cleared up.

If it be remarkable enough, as it surely is, that patients should undergo mercurial frictions for months together without being ptyalised, there is something else which is still more curious. The attendants who carry out the frictions are not allowed to use any glove (as this is supposed to render the inunction much less complete), but work always with the naked palm; they are in the habit of performing inunction on many separate patients in the same day; yet they do not appear ever to become salivated, or show any other signs of mercurial poisoning.

[That the fact is so, we know, not merely from Dr. Brandis's statements, but from private inquiries which we have ourselves made.]

No doubt the thickness of skin upon the palms is one hindrance to the absorption of mercury into the rubber's system; in part it must also be attributed to the fact that the palms are *soaped* before the inunction is begun, and well washed with soap and water when it is finished; still, when we consider that each of the rubbers performs from ten to fifteen inunctions daily, their universal immunity from mercurial poisoning seems very remarkable.

Dr. Brandis lays great stress upon the necessity of giving the patients thoroughly pure air to breathe, and an excellent nutrition, during the progress of the inunction treatment. He speaks

strongly of the disastrous results which followed the old plan of salivation and residence in a heated room during the process.

As regards the part which the internal use of the sulphur waters plays in the cure of syphilis, Dr. Brandis believes this is entirely in virtue of their action in producing watery purgation—their eliminative action, in fact. We confess to a very strong suspicion that this is an error, and that sulphur eliminates not one particle of syphilis, though it may (possibly) eliminate some mercury which is in dangerous excess. That purgation, *per se*, in anywise helps the cure of constitutional syphilis we steadily decline to believe, for we have seen the subjects of this disease submitted to the severest treatment of that nature, with the invariable result that harm, and not good, was done. In short, it may be doubted whether the Aix waters are in anywise effective against syphilis, save by virtue of their heat: and it is plain enough that the main cause of success, at least in Dr. Brandis's hands, is the employment of an unusual, careful, and prolonged mercurial treatment, together with special attention to hygiene and nutrition. One very interesting matter is the way in which the author speaks of iodide of potassium. He does not seem to think it safe even to trust to the use of the iodide alone, but for three reasons he thinks it an indispensable element in treatment: first, because of its utility in bone and periosteum affections; secondly, because of the possibility of pushing its use rapidly, in large doses, when danger is imminent; and, thirdly, because of its great influence at those periods when mercury, having benefited for a time, has temporarily lost its curative power.

We have personally verified, with much curiosity and interest, the effects of the Aix treatment in Dr. Brandis's hands; and in the first place we may say, unreservedly, that as far as immediate effects go, the treatment is remarkably, we might say startlingly, successful. We could more especially speak of several instances in which most lingering cases of syphilitic paralysis have been entirely or almost entirely cured: but we prefer to devote our limited space to one narrative, which is not merely interesting as regards the results of treatment, but exhibits the type, we suspect, of a considerable class of misunderstood cases which are in reality due to syphilis.

A gentleman, aged 32, single, applied to us for advice early in March last, suffering from severe and intractable dyspepsia, which had been, on the whole, constantly and steadily increasing for the last two years, but also was distinctly traceable back to an earlier period, which will be presently referred to. There was no doubt, from his story, that various minor circumstances concurred to aggravate the dyspepsia from time to time: excitement and excess of any kind (he did not exceed in *drink*) would at once bring on a bad attack; but independently of

this there were almost constant uneasiness at the epigastrium, flatulence, and regurgitation of a portion of any food that might be taken. He was low-spirited and hypochondriacal, and had lost flesh a good deal; said that he had consulted a number of medical men with very little benefit, until three months before his visit to ourselves, when a physician prescribed Easton's syrup. Although this could not be continued for more than a week (as the strychnia produced uncomfortable effects), the patient felt better, and travelled abroad during some weeks succeeding this, with comparatively little discomfort. Almost immediately on his return to England, however, the dyspepsia returned, attended with much agitation and inability to sleep, from the feeling of great discomfort in the stomach.

Following somewhat the lead of his former physician, we commenced the treatment of this patient with nux vomica and bismuth; but he made no progress. We then inquired more particularly into his history, and the following particulars appeared. His health was "magnificent" up to seven or eight years before, when he caught syphilis, and had secondary eruptions: the disease appeared to subside under treatment: nevertheless, he said, he had not been the same man since; and his tendency to dyspepsia began to show itself. Three years ago he got a herpetic rash, sore throat, and other symptoms of what seemed to have been either "mumps" or catarrh, but was followed by an obstinate purulent discharge from the left nostril, which lasted almost down to the time of his visit to ourselves: he at last suppressed it, as he believes, by the use of snuff, but (whether *post hoc* or *propter hoc*) he immediately afterwards got "irritable red-brownish blotches" all over the backs of the hands, the wrists, ankles, legs, and trunk. The patient himself suggested, rather earnestly, that the syphilitic taint was the true origin of the dyspepsia, for that the tendency to the latter had always existed since the constitutional disease, though he had been perfectly free from it up to that time. We yielded to his suggestion, and administered $\frac{1}{2}$ grain of hyd. bichl. *ter die*. The improvement which he made was remarkably rapid; not merely did the regurgitation and other dyspeptic symptoms diminish, but he expressed himself as greatly relieved from his mental depression. After about a week the bichloride was reduced to $\frac{1}{8}$ gr. *ter die*; he took this for another fortnight, and then finally went to Aix-la-Chapelle to finish his cure, as certain traces of the dyspepsia clung to him still. Here he stayed, on the whole, for about five weeks (though in two visits, broken by a journey to England on business), and was submitted by Dr. Brandis to the mercurial inunction. He had, in all, thirty-seven rubbings.

On June 2 this patient called upon us, looking the picture of

health and animation. He joyfully exclaimed that he had lost every trace of dyspepsia and of mental depression; that for the first time for seven years he could eat, with perfect impunity, any quantity and any kind of food. On June 2, having had a slight relapse of dyspepsia, he was placed on pot. iod. gr. lx. per diem, now reduced to gr. xl. per diem, as he is almost entirely well.

Concerning the mercurial treatment of this patient at Aix, we made very close inquiry, and we found that he undoubtedly was salivated, though not in a very high degree: so that Dr. Brandis's own rule of treatment was (unintentionally, no doubt) departed from in this instance. This leads us to remark that, from what we have heard from other patients who have been to Aix, it is difficult not to believe that ptyalism is really produced by Dr. Brandis's treatment with somewhat greater frequency than he supposes or intends. But to return to the case of our patient, nothing could well be more striking than his account of the effects of the inunction treatment. He graphically described the immediate and extraordinary increase of appetite which it produced; though lean enough, he ate so enormously, that as soon as ever the active treatment was suspended he made weight with the greatest rapidity. Above all, he insisted on the fact that his previous depression of mind was succeeded by a remarkable cheerfulness, so that Aix (which assuredly is not one's *beau idéal* of a lively residence) appeared to him quite a fascinating place.

Before recurring to the general question of prolonged mercurial treatment for constitutional syphilis, we must conclude what we have to say on the probably syphilitic nature of a not inconsiderable class of dyspeptic cases. Our experience with the above-mentioned patient has caused us to look back upon other examples of constitutional syphilis, especially as affecting the nervous system, which had previously been under our care; and it has struck us that dyspepsia, of an obstinate and intractable kind, appears to have been present in a proportion of cases far more considerable than we had noticed at the time: and that this dyspepsia was often *suddenly* relieved by the anti-syphilitic treatment. We shall not attempt to theorise on the pathology of this obscure complication: but it certainly deserves very close attention from all those who have opportunities of studying the dyspepsias on a wide scale.

We cannot but think it is of the highest importance that the profession should come to a careful decision upon the question whether the very striking results unquestionably obtained by the treatment which we have been discussing deserve to be considered in the light of permanent cures, and whether, if so, it would have been impossible to obtain as good results in another

way. We should note here that Dr. Brandis anxiously protests that a single course of inunctions, even if somewhat prolonged, will in certain cases prove insufficient, and that the patient must return a second and possibly a third time to the cure. That seems a rather strong step, yet we are not at all prepared to say, dogmatically, that it would put the constitution in serious danger of permanent damage. We cannot but observe, however, that we are dissatisfied as to the way in which iodide of potassium is discussed by Dr. Brandis. So remarkable are the effects obtained in numerous cases of tertiary syphilis by English physicians, that we cannot but suspect that at Aix the remedy is handled timidly, instead of being given, as is now the custom in London, in very large doses. Otherwise it would have been impossible for Dr. Brandis to say that syphilis never gets well with iodide of potassium alone; unless, indeed, he is speaking of an absolute and final cure. In the latter case, are we to understand him as affirming that patients who have once been properly and thoroughly treated with mercury are thenceforward safe from recurrences of syphilitic symptoms? We confess that we believe no such thing, for our own part. It is true enough that patients who have shown symptoms of constitutional syphilis have been known to pass long lives without the recurrence of any such phenomena: but then, this has happened in quite as remarkable instances where there has been no treatment as when there has been mercurial treatment: although we freely grant that it has more commonly taken place when rational and moderate mercurial medication has been adopted than in opposite circumstances. But grant the presence of sundry other factors—want, exposure, habitual over-exertion, or debauchery—and no man, we honestly believe, can guarantee a once syphilised person against disastrous recurrences of the disease. We have seen numerous cases in which respectable men, fathers of families and leading perfectly decent lives, have been attacked with one or other of the nervous forms of tertiary syphilis from twenty to twenty-five years after the last appearance of any symptoms of the constitutional disorder, and notwithstanding that they had been most fully (and apparently successfully) treated with mercury for the primary disease. We have recently resigned the charge of an unfortunate woman, who, during a constitutional syphilis of twenty years' standing, has been treated with mercury on at least a dozen occasions: once or twice severely, the other times mildly and continuously: yet she never goes more than three months without a relapse. And upon this point we do not doubt that Lancereaux¹ is right when he says that mercury and iodide of

¹ *Treatise on Syphilis* (Syd. Soc. Trans.), vol. ii. p. 334.

potassium only act upon the anatomical consequences of syphilis, not upon the essence of the disease, which can only be triumphed over (if at all) by the natural processes of the organism.

As regards the share which sulphurous and alkaline waters can really be supposed to take in the cure of constitutional syphilis, it must finally be confessed that our knowledge is very vague. It is obvious that Dr. Brandis's treatment *may* very well be effective without any direct intervention of the influence of the waters of Aix : and when we turn to other experienced authors, such as Lancereaux,¹ we find them very unwilling to make any but the most indefinite (though sometimes strongly affirmative) answers on this point. For the present we suppose we have scarcely a right to deny that the sulphur waters do occasionally hasten disintegration of tissues, and may consequently give a start to the process of absorption.

¹ Op. cit.

Clinic of the Month.

Surgical Treatment of Aneurism.—In the capital lectures on this subject now in course of publication in the *Lancet* by Mr. Holmes, it is maintained that the modern method of ligature with catgut cut short and buried in the wound, though excellent and greatly superior to the old methods, does not preclude compression. Compression, Mr. Holmes thinks, may even be employed in cases of carotid aneurism by the exercise of a little patience and perseverance, both on the part of the patient and of the surgeon, and should be adopted in preference to the ligature on account of the fatal results attending this last mode of treatment. Mr. Holmes is further of opinion that when the ligature of the carotid becomes necessary, it may often be advisable to evacuate the contents of the sac and secure the distal end of the artery; that Brasdor's operation on the carotid artery, though very rarely indicated, yet rests on sound anatomical and surgical principles; that traumatic aneurisms of the vertebral arteries are often, though they need not and should not be, confounded with aneurism of the carotids, and that they may very probably be successfully treated by compression or by the old operation; that under the term "orbital aneurism" several affections are included, some of which are either spontaneously curable, or are curable by milder means than ligature. He believes it to be possible that in rare cases intracranial aneurism may be diagnosed and successfully treated, that arterio-venous aneurism in the neck is usually harmless; and he maintains that in view of the fatal results that so often accompany ligature of the subclavians, the treatment by compression should be tried in cases of axillary aneurism. Upon the effects of manipulation in the treatment of this aneurism he thinks we possess no trustworthy evidence. Brachial and antibrachial aneurisms, whether traumatic or spontaneous, are amenable in his opinion to the treatment by compression. (*Lancet*, June 14, 1873.)

On the Enlargement of the Tonsils as a Cause of Nightmare. J. W. Haward remarks that various evils have

been attributed to the enlargement of the tonsils, some of them perhaps more imaginary than real; but it occurred to him, after seeing several instances of distressing nightmare in children, that it was clearly referable to the condition of the tonsils. The case which first led him to observe this was a very striking one. An intelligent and not at all nervous girl of thirteen years had for several months been subject to occasional attacks of nightmare, which were increasing in frequency and severity. A short time, usually about an hour, after going to bed, the child arose with a loud scream, and, on the parents going into the room, was found sitting up in bed, the eyes vacantly staring, and the face wearing an expression of extreme alarm. Although the eyes were open, she did not appear to be awake, and required moving and loudly speaking to before she seemed to appreciate the presence of those around her. She would then give a sigh, say that she had been frightened, she did not know by what, and presently fall asleep again. These attacks occurred sometimes several times during the night, and for several nights in succession; then were absent and then returned again, perhaps for some weeks. She was a healthy-looking child, and had been nurtured with every care, and the parents were much distressed by these symptoms, fearing they might be premonitory of some serious cerebral affection. She had been treated without benefit by various medicines. She was taken to see Mr. Haward on account of an attack of stomatitis, and the parents then mentioned the occurrence of the nightmare. On examining the mouth it was found that the tonsils were greatly enlarged, and it seemed possible that the nightmare might depend on the obstruction to respiration thus produced, with the consequent non-aëration of the blood, and cerebral congestion. This idea was confirmed by the mother saying that she snored loudly, and that the attacks were generally worse when she had a cold. As soon as she recovered from the stomatitis a portion of the tonsils were removed, and from that time she never had an attack of nightmare. Mr. Haward has since this noted three similar cases, all children, in each of which, when the tonsils had been removed, no recurrence of nightmare took place. Under these circumstances, when a child suffers from night terrors it would be well to inquire into the state of the tonsils as a probable cause. The kind of nightmare thus produced seems to differ from that having its origin in gastric irritation or dentition, chiefly in this: whereas this last kind occurs as a rule only once in the night (as Dr. West points out), and the child then sleeps quietly, that due to enlarged tonsils often recurs several times in the same night, and is invariably observed to be aggravated by the child catching cold. (*British Medical Journal*, June 7, 1873.)

Injectiōns of Spirits of Wine into Adipose Tumours.—

Dr. Hasse, of Wordhausen, recommends the injection of alcohol into the substance of adipose or lipomatous tumours. He gives one case of a female who had a large fatty tumour reaching from the shoulder to the armpit. He injected alcohol into this at four different sittings, with fifteen days' interval, directing the syringe into different parts of the tumour. In a few days there was some slight inflammation. The tumour became at first firmer, but soon softened and fluctuated at several points. In three weeks an incision was made into the tumour, and the fatty matter could be pressed out in a liquid form. In another case of a man aged forty, similar treatment was adopted with similar result. (*Med. Press and Circular*, May 21, 1873.)

Chloral in Gout.—A correspondent of the *Med. Times and Gazette* writes that he has been a victim to gout for ten years, and knows its symptoms well. On no previous occasions have its pangs relaxed their hold under a week, and on their last visit they lasted fourteen days. During that memorable fortnight of misery there was one oasis procured by chloral. It was by far too delicious to be forgotten. On April 30 the gout seized the right great toe. Experience induced him to believe that this was the beginning of a prolonged bout. At bedtime he took about seventy grains of chloral (he did not measure the solution lest he should be frightened). Three nights afterwards he did measure, and believes that his first dose was nearly eighty grains. He slept profoundly all night, had milk and brandy for breakfast, and slept on; awoke and transacted some business, dined on steak and brandy-and-water, and slept again. During this period, singularly enough, the pain in the foot was intense enough to draw tears if not sleeping. A second dose of chloral the second night and a similar second day followed, the gouty inflammation having extended over the whole dorsum of the foot. Many a time, when agony was at its height, a glass of milk and brandy soothed it, and sent him to sleep. So the days passed. He took again his usual dose of chloral at night, and the next day was able to get out of bed, but not able to walk. The pain and gout have for a while gone; he has slept them off in five days. Before trying this he had tried every known form of opiate uselessly.

Alkalies in Burns.—Dr. D. B. Dalzell says, in looking over some papers of a deceased friend (who was an excellent chemist), that he has come across some remarks on the treatment of burns, scalds, and bruises by the local application of alkalies. So long ago as 1841 he was in the habit of applying the water of potassa in such cases, and when used early he never once saw

it fail in removing the pain "in a few minutes," and effecting a cure "almost like magic." He left the injured surface exposed to the air as long as any pain was felt, having always found that covering it from the air prolonged the pain and retarded the cure. He instances a case of scald with boiling water, in which the potassa was not applied until some hours after the accident. Small blisters had risen, and the skin was highly reddened and the pain severe. Having only a small supply of alkali, in order to make it go further the surface was covered with linen, which was moistened from time to time with the lotion; hence the pain was not removed for at least six hours. This, he says, was the only case he ever had in which the pain was not got over in less than one hour. The lotion should be applied with a feather, leaving the injured part uncovered. The aqua potassæ which he employed was of the strength of the Pharmacopœia, and he prepared it himself, as he found it difficult to get it good in shops. In regard to the *rationale* of the cure:—Scalds, burns, and bruises occasion decomposition of the blood of the injured part. In all animal decomposition (especially in that of blood and coagulable lymph) nitrogen is given off, and combines with oxygen, forming perhaps hyponitrous acid, which occasions much of the pain felt. Now, if an alkali such as liquid potassa be applied, it quickly aids the formation of nitric acid, and combines with it. Thus nitrate of potash, a cooling salt, is formed, which aids in abstracting the heat; and by this and the antiseptic constringing action of the alkali on the animal texture the progress of decomposition is speedily arrested, and nature is left at liberty to heal the injury. This may explain, Dr. Dalzell thinks, the benefit which has undoubtedly in some instances been obtained from the application of alcohol, turpentine, and such like in burns. Alcohol prevents decomposition, and animal substances are preserved in it for years. May not, he asks, the beneficial action of carbolic acid and oils be explained on this principle? When a large surface is denuded of cuticle, or a very vascular part is injured, it may be necessary to dilute the alcohol or to apply it in the form of a soap, by mixing about two parts of aqua potassæ with one of olive oil, or a still larger proportion of oil. (*Med. Times and Gazette*, May 17, 1873.)

Extracts from British and Foreign Journals.

Therapeutical Value of Phosphorus.—In an interesting paper on the physiological action and therapeutical effects of phosphorus, published in the *Bulletin Général de Thérapeutique*, M. Gubler states that phosphorus is a diffusible stimulant of great energy and of dangerous activity. It should therefore only be prescribed with the greatest possible caution, and certain contra-indications may in the first instance be laid down. Thus it should not be used in any affection characterised by nervous, circulatory, or trophic excitation, as in tonic and clonic convulsions, contractions, neuroses having a hypersthenic origin, diffuse peri-encephalitis with general paralysis, phlegmasiæ of all forms, fevers of every kind, exanthematous affections, &c. The indications for its use are the existence of disease unaccompanied by inflammation, fever, and nervous excitation, and especially in such cases as are characterised by depression of the circulation, either local or general, diminished power of generating heat, exhaustion or local asthenia, with paralysis of sensation and movement. Hence its value in cachectic states consecutive to long and exhausting diseases, marsh fevers, protracted convalescence, tabes dorsalis, paralyses of old date, and of cerebral, medullary, or peripheric origin, when there are no signs of irritation; in hemiplegia, paraplegia, amaurosis, and other partial paralyses. Phosphorus again is sometimes useful in making chronic eruptions advance or recede; but it is especially as a remedy for impotence that it has been praised, though it has often disappointed the expectations of those who have prescribed it, and has either proved of no value at all, or its effects have only been ephemeral. Hence it would appear that the real remedial power of phosphorus is considerably restricted, and that it can only be regarded as of great value in paralytic affections. Dr. Delpech, who has studied so deeply the effects of sulphuret of carbon, praises phosphorus highly as an agent to remove the paralysis and loss of power which accompany intoxication by that substance. It is also of service in the so-

called rheumatismal and hysterical paraplegia, or in other words those forms of paraplegia which are not caused by organic lesion, as well as in cases of cerebral disease in which all irritation has ceased and cicatrisation has taken place. It is still more strongly indicated in cases of asthenic and diffused paralysis, consequent on diphtheria or some other acute affection. Gueneau de Mussy, Isambert, and Féréol have all found it efficacious in the treatment of mercurial tremor; it is also believed to be so in paralysis agitans and in the various forms of medullary sclerosis affecting the antero-lateral cords, and above all in sclerosis of the posterior columns, the symptomatic expression of which is summed up in the term locomotor ataxy, which has been applied to it by Duchenne. Dujardin-Beaumetz is he who has most strongly recommended the plan of treatment by phosphorus in this and similar affections; but it is questionable, M. Gubler thinks, how far many of the successes attributed to its use are really due to its remedial powers. We forget, he says, the natural processes of cure that often take place. Phosphorus is an active agent that may momentarily re-illumine the fading spark and revivify the languishing powers of life; but as it brings no energy with it, it impoverishes rather than enriches, and can do little for a nervous system exhausted by a chronic affection. The amorphous phosphorus is perhaps the best mode of prescribing it, as this possesses no exciting or irritating action. Externally, it has been chiefly employed in squamous affections of the skin as a parasiticide in itch, and as a caustic in the place of the moxa. Recently Tavignot has declared that it will render the cataractous lens transparent, but the negative facts obtained by MM. Gosselin and Maisonneuve render this more than doubtful. In regard to the mode of its administration, solutions are usually preferable to pills. Amongst the former are the ethereal tincture, which contains one part in sixty, and of which ten drops are a dose; the solution in chloroform, which is now almost abandoned; and the solution in oil, which is by far the best, and especially that prepared by Méhu. Here the oil is dehydrated and decolorised by exposure to a heat of 250° C., and the phosphorus is added when it has cooled. A twentieth part of ether is then added. The proportion is one part of phosphorus to 500 of oil, and it contains two milligrammes in fifteen drops. Each capsule contains one milligramme. (*Bulletin Général de Thérapeutique*, May 30, 1873.)

A New Method of injecting Medicated Fluids into the Drum Cavity for the Relief of Deafness.—Dr. Chisholm reports the case of a gentleman, aged 23, from Texas, who has been so deaf for the past three years, in the left ear, that he can hear absolutely nothing with it. Knowing himself to be completely

deaf in this ear, and believing his case incurable, he went to Baltimore for treatment, solely to gratify his friends at their earnest solicitation. Upon examining the right ear, it was found perfect. In the left ear the drum cavity was nearly effaced, with the drum membrane firmly adherent to the inner osseous wall or promontory of the cochlea. The Eustachian tube admitted air into this nearly effaced tympanic cavity, as perceived through the otoscope, yet the ticking of a watch could not be heard when pressed against the ear. The object to be attained in this case was to liberate the drum membrane and restore the drum cavity. This was in a measure effected by the following novel treatment:—Having secured the Eustachian catheter in the pharyngeal orifice of the Eustachian tube, a small rubber tympanic catheter (Weber's) was introduced through the silver Eustachian tube, until its free end entered the cavity itself. Through this channel the drum cavity was filled with a solution of sulphate of copper of the strength of four grains to one drachm. This was injected for the purpose of exciting inflammatory action, its effects causing the old bonds of adhesion to soften. The injection was repeated each day. After a few days, air was driven with sufficient force through the tympanic catheter to stretch the adhesive bands and allow the depressed membrane to resume somewhat its normal position. The result was an immediate improvement in the hearing. From absolute deafness, in the short space of a week, the patient could hear the watch at six inches from the ear, and could understand conversations as well with this formerly deaf ear as he could with the perfect one. This result was unusually good. (*Baltimore Annual Surgery Reports*, 1873.)

Hydrocyanic Acid as a Remedial Agent in Delirium Tremens.—Dr. Daw, in the *British Medical Journal* of May 31st, in speaking of this remedy, states that the first and most important symptoms of delirium tremens are loss of appetite, followed by nausea and vomiting, and accompanied by mental excitement, as exemplified by anxious and often hideous dreams during sleep, sometimes wandering; and during wakefulness, by the presence of imaginary objects and foes. The tongue is furred, indicating a disturbed condition of the digestive organs, and tremulous in sympathy with the impaired state of the nervous centres. The circulation, which often at the onset of an attack is depressed, becomes quickened, and oftentimes the patient becomes violent, as in the delirium of an acute attack of mania. The course of the disease may be from bad to worse, until exhaustion and death end the scene. Or, on the other hand, sleep may supervene, and the sufferer awake refreshed and in a fair way towards recovery.

Opium in its various forms, digitalis, and belladonna have been used in the treatment of this disease, but they do not fulfil all the indications of treatment, and are liable to cause harm, either by their cumulativeness or by their influence on some idiosyncrasy of the system. Hydrocyanic acid does unquestionably fulfil these indications. It allays the irritation of the stomach and checks the nausea and vomiting; it quiets the nervous excitement, and by so doing tends to produce sleep; and it also controls the action of the heart. It has the advantages of producing its effects quickly, and not being cumulative, and is taken readily by most people. Dr. Daw has used it with most satisfactory results, and mentions, as his usual method of administration, a combination with bicarbonate of potash, chloric ether, and camphor mixture, in doses of one, two, or three minims of the Pharmacopœia solution, every two, three, or four hours, according to the severity of the case; and also that benefit may also be derived sometimes from the addition of either three or four grains of carbonate of ammonia, or a few minims of the compound spirit of ammonia. The patient is to be nourished by the administration of beef-tea, milk, &c., and wine, or other alcoholic stimulants to be given, according to the discretion of the medical adviser; the less, however, the better. As soon as the worst symptoms have been relieved by the above treatment, the appetite is soon restored by the use of dilute nitric acid and decoction of cinchona. (*British Medical Journal*, May 31, 1873.)

Contribution to the Study of the Action of Vomiting.—Dr. Antonio d'Ornellas states that for some time past he has been engaged in investigating the action of emetine, and he publishes some conclusions in regard to the physiology of the act of vomiting, to which he has been led in the course of these inquiries. Vomiting, by which the contents of the stomach are evacuated by the mouth, can be induced in various ways, though the special sensibility leading to its occurrence is only possessed by the mucous membrane of the stomach itself. The act can be completed either by the intrinsic action of the stomach, by the extrinsic action of the expiratory muscles, or most frequently by the synergetic action of both the stomach itself and these muscles. The stomach is supplied by the pneumogastric and by the sympathetic nerves. The former is really composed of two nerves, representing a spinal nerve, of which the anterior or motor is the spinal accessory, and the posterior or sensory, the pneumogastric; and it may be regarded as establishing an insensible transition between the ordinary nerves and those of organic life. The posterior or vagus nerve conducts the sensations of nausea, hunger, thirst, &c.; the anterior or spinal

accessory governs the movements of the stomach, which are ordinarily called into play in the acts of chymification; the muscles which produce the extrinsic efforts of vomiting (the expiratory muscles) receive their nerves from the spinal accessory, from the fourth cervical (phrenic), and from the first two lumbar nerves. M. d'Ornellas' investigations have led him to believe that emetics are eliminated by the stomach and intestines, and that they provoke vomiting by exciting the peripheric expansion of the nerves—their terminal filaments—and not by primarily irritating the nervous centres. In the first place, his experiments on dogs show that when emetine is injected hypodermically in dogs, an alcoholic extract of the stomach and intestine and of their contents is capable of inducing vomiting in pigeons—which proves that it is eliminated in this way from the body: secondly, emetine, when injected hypodermically, not only acts very gently, but takes about *three times* longer to act than when directly ingested into the stomach: lastly, vomiting takes place as soon as its elimination by the stomach commences, for if a dog be stunned by the hypodermic injection of a very large dose of the alkaloid, the inflammatory lesions of the stomach and intestines show that the elimination has commenced coincidently with the first efforts to vomit, that is to say about forty minutes after its injection. Moreover, both in man and in the dog, vomiting is preceded by abundant salivation, which is probably to be regarded as a sign of the elimination of the poison through the salivary glands. Again, where large doses are given and violent vomitings follow, severe inflammations of the stomach and duodenum are produced; whilst if the doses subcutaneously injected are small, no vomiting may be produced, and inflammatory lesions are only found in the jejunum and large intestine. As a rule, the seat of the lesions approximates the lower extremity of the digestive tube in proportion as the doses have been frequent and small. Similar conclusions to the above have been arrived at in regard to the action of potassio-tartrate of antimony in cats, by MM. Kleimann and Simonowitsch. Thus it would appear that vomiting is either a reflex action provoked by the *direct* action of the emetic substance on the sensory extremities of the vagus, or by indirect action on sensory nervous filaments distributed in other regions, as occurs where it is produced by the sight or smell of disagreeable objects. Before admitting that the pneumogastric is the principal agent in inducing vomiting, M. d'Ornellas proceeded to perform other experiments. In several dogs he divided the pneumogastrics simultaneously on both sides of the neck, and constantly observed that vomiting immediately occurred, the section having in fact excited the centric extremity of the vagi. When these efforts had ceased,

emetic doses of emetine were injected into the cellular tissue, and he now found either that vomiting did not take place at all, or that it occurred very late, as much as three hours after the operation, and then only to a very small extent. On examination, the same gastro-intestinal lesions as before were found, and hence it would seem that the nervous path which previously existed had been intercepted. But it may be asked, why should vomiting take place at all after section of the pneumogastric? and the only reply that can be made is that given by Claude Bernard, that nature is not restricted to one means of effecting her object, but that the sympathetic may here take on the functions of the vagus, and act vicariously as the centripetal excitator. M. d'Ornellas believes that all the modes of vomiting admit, on the grounds just given, of easy explanation. Thus, vomiting induced by tickling the fauces; the vomiting occurring in cancer or ulcer of the stomach; in gastric derangement, however produced; in abdominal diseases, as hepatic and renal calculi—act through the pneumogastric: whilst the vomiting of meningitis, of cerebral tumour, of hæmorrhage, all act by increasing or diminishing the vascularity of the medulla oblongata. (*Bulletin Général de Thérapeutique*, 5^{ème} livraison, 1873.)

The Temperature in Epilepsy and Hysteria.—Dr. Bourneville states that he has satisfied himself, from observations taken in the Salpêtrière and St. Louis Hospitals, that during an attack of epilepsy the temperature augments; so also in attacks of epileptiform hysteria the temperature rises: on the contrary, in attacks of pure hysteria there does not appear to be any increase. He thinks that by this means it is possible to discover an attack of true from feigned epilepsy. Where epilepsy is simulated, the attacks are always the most severe possible, yet the temperature does not rise above the physiological degree; but when the real attacks are severe, the temperature not only rises during the fit, but remains above the normal for several hours. (*Le Mouvement Médical*, No. 11.)

Treatment of various Uterine Diseases.—1. *Ulcerations of the Neck of the Uterus.*—M. Lucas-Championnière states that at the Bureau Central des Hôpitaux, where consultations take place twice a week on a large number of women, he has had the opportunity of hearing some of the clinical remarks of M. de Saint-Germain. As a general rule this physician rarely employs canterisation for ulcerations of the neck of the bladder; and he avoids them on two accounts: first, because there are various inconveniences that arise in cases where, as in the patients that present themselves at these consultations, many of whom

come from a distance, the women are unable to rest immediately after the application of the cauteries; and secondly, because he thinks other means can be adopted in their stead. M. de Saint-Germain makes great use of medicated bags (*sachets médicamenteux*). These are small cylinders, five or six centimetres in length (3 inches), a little larger than the thumb, made of gauze, enclosing dry linseed meal. These are introduced and allowed to remain in the vagina after having been dipped in glycerine mixed with various remedial agents. When he treats ulcerations of the neck, which is itself more or less hypertrophied, and accompanied by more or less discharge without much pain, the sachet is dipped in a fluid composed of a solution of 12 parts of tannin in 100 of glycerine. This is pushed on to the extremity of the speculum, and applied to the neck, being kept in position by some charpie which is well pressed in. It is allowed to remain in position for three or four days, and then withdrawn by means of a thread attached to the bag. The vagina is well washed out, and a fresh sachet introduced. This simple mode of dressing is very useful in a large number of other cases, as for example in vaginitis. It can be advantageously applied after slight cauterisation has been made with nitrate of silver or other substance. It may even prove of service in cases of retroversion. It is then, however, only a palliative, but the introduction of a plug dipped in glycerine and tannin into the posterior cul-de-sac gives great and immediate relief. Where the ulceration is accompanied by great pain, the same kind of sachet is introduced, only dipped in a solution of 8 parts of extract of belladonna in 100 of glycerine, instead of the tannin and glycerine. One of the patients complained of severe abdominal pain, of acute pain during intercourse, and of abundant discharge. Vaginal examination demonstrated the presence of a slightly enlarged neck of the uterus, with slight ulceration, very free uterine catarrh, and the hystrometer showed that the cavity of the neck was enlarged, and brought away a little blood. M. de Saint-Germain prescribed (1) that the patient should take every morning fasting a teaspoonful of white mustard-seed, in half a tumbler of fresh water; (2) a starch bath (*bain amidonnée*) every other day; (3) injection of infusion of walnut leaves (*feuilles de noyer*) three times a day; (4) linseed-meal poultices to the belly every night on retiring to rest; and lastly, of hop tea three glasses a day (*tisane de houblon*).

2. *Leucorrhœa*.—In severe cases of leucorrhœa without manifest lesion, M. de Saint-Germain simply orders sulphur baths and free injection with the water of the bath. He has obtained great advantages from this, and even recommends it for young girls, using a sufficiently small canula.

3. *Cancer*.—The following prescription was ordered for a case

of uterine carcinoma. It comprehends disinfectants, tonics, and also iodide of potassium, so that in the event of the surgeon being mistaken in his diagnosis no chance may be lost:—(1) Injections of chlorinated water three times a day; (2) a sulphur bath every other day; (3) a spoonful of solution of iodide of potassium internally every morning and evening, containing a grain or two of the salt; (4) every morning fasting a spoonful of cod-liver oil; (5) infusion of gentian, three glasses per diem. (*Journal de Médecine et de Chirurgie*, Lucas-Championnière, tome xliv. 1873, 2^e cahier.)

The Use of Nitric Acid in the Treatment of Uterine Disease.—Dr. Lombe Atthill, of Dublin, observes that most British gynecologists are convinced of the advantages to be derived from the application of remedies to the inner surface of the uterus when the mucous membrane lining the cavity of the organ is in a diseased condition. Careful consideration of the various cases, and of the remedies that have been proposed, renders it probable that no single method and no single agent can be satisfactory in all cases. In the paper from which this extract is taken, he endeavours to point out the advantages in certain cases of the application of fuming nitric acid. The first case he mentions was one where the patient, a young woman, suffered from profuse menorrhagia. The cervix uteri was soft and swollen, the vaginal surface deprived of its epithelium and covered with large vascular papillæ, the os uteri patulous, and the mucous membrane lining the cervical canal being also evidently in a diseased condition. The solid nitrate of silver was repeatedly applied without any improvement following. Then, suspecting that the disease extended into the uterine cavity, Dr. Atthill introduced a sea-tangle tent, on withdrawing which he passed up to the fundus a stilette armed with a film of cotton saturated with the fuming nitric acid. No pain was experienced, but improvement immediately commenced, and resulted in perfect recovery. He describes other cases of profuse menstruation in which he applied the same remedy with more or less advantage. He has found this plan of treatment to be very valuable when applied with the view of checking or preventing the occurrence of hæmorrhage after the removal of intra-uterine tumours, or of exciting healthy action in the mucous membrane lining the cavity of the uterus which has been the seat of polypoid growths. In Dublin nearly all obstetric practitioners now mop out the interior of the uterus with the fuming nitric acid after the removal of tumours. In one case, however, which Dr. Atthill records, it caused occlusion of the os and cervix uteri. He thinks the fuming nitric acid very useful in those forms of imbedded fibrous tumours of the uterus in which, hæmorrhage being profuse,

surgical interference seems called for. His experience in the use of the fuming nitric acid has led him to the following conclusions. 1. That when tenderness on pressure exists, it should, before the acid is applied, be removed, or at least materially lessened, by local depletion. 2. That when this precaution has been taken, fuming nitric acid may be applied with safety to the interior of the uterus. 3. That when the cervix has been previously freely dilated, its application does not cause any pain. 4. That in some instances it seems to exert a directly soothing effect upon the nerves of the uterus. 5. That when applied through a canula pain is sometimes produced, but less severe in character than that caused by the use of the solid nitrate of silver. 6. That its use is in some cases followed by hæmorrhage of moderate amount, which however does not influence the result of the case. 7. That if applied to the *healthy* cervix, it may produce contraction and possibly obliteration of the cervical canal, and that consequently means should be adopted to guard the cervical canal, when *healthy*, from its action. 8. That in cases where imbedded fibrous tumours exist, the fuming nitric acid exercises a marked influence in controlling hæmorrhage and allaying pain. When it has been decided that nitric acid shall be applied after previous dilatation of the cervical canal, Dr. Atthill seizes the anterior lip with a vulsellum, and thus draws down and steadies the uterus. He then introduces the blades of his intra-uterine speculum to the depth of about an inch, and expands them slowly to an extent sufficient to permit a pair of fine forceps holding a roll of cotton to be introduced. With the cotton he dries the inner surface of the uterus, and, withdrawing the cotton, passes through the speculum a probe armed with a roll of cotton saturated with the fuming nitric acid. A second probe, similarly armed, is passed to ensure thorough cauterisation. The blades of the speculum are now closed. A pledget of cotton soaked in oil, or, better, in glycerine, is placed in the vagina, and the patient kept quiet in bed for some days. (*Obstetrical Journal*, No. 3, June 1873.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

HOMŒOPATHIC PILULES.¹—As shown in the previous note, we failed to detect either strychnine or atropine in pilules of the second dilution of strychnos nux vomica and belladonna respectively, although both alkaloids should have been fairly within the reach of analysis. Since then we have examined some pilules of aconite and belladonna of the first dilution, which contain professedly one part by weight of the drug in 100 parts by weight of the pilules. The results are also entirely negative.

Aconitum Napellus Pilules.—First sample: no aconite could be detected in 100 of the pilules (weight 80·85 grains).

Second sample: same result (weight of the 100 pilules, 96·40 grains).

Comparative experiments showed that had these pilules contained as small a quantity even as $\frac{1}{8000}$ of a grain of aconite, it would have been detected with certainty. This corresponds to about the 160th part of a grain of the extract of aconite; the quantity which should have been present is from $\frac{1}{4}$ to nearly 1 grain, or more than 100 times more.

Belladonna Pilules.—First sample: no atropine could be detected in 100 pilules (weight 80·39 grains).

Second sample: same result (weight of 100 pilules, 81·00 grains).

Comparative experiments showed that by means of chemical tests alone, as little as $\frac{1}{8000}$ of a grain of atropine would have been detected with certainty. This corresponds to about $\frac{1}{80}$ of a grain of the extract of belladonna, whereas as much as $\frac{1}{4}$ of a grain, or 24 times as much, should have been present. As the chemical test showed conclusively that, to say the least, much less of the drug was present than the pilules professed to contain, it was not considered necessary to make use of the still more delicate physiological test. We should thus in this case, as well as in the former one, be obliged to place any cures,

¹ Continued from the April No.

should there be such, following the use of such pilules, entirely to the credit of the imagination.

The samples of copper and mercury pilules, two each, mentioned in the first note, were obtained from Messrs. Leath and Ross, 9, Vere Street, and from Messrs. J. Epps and Co., 170, Piccadilly. The pilules of nux vomica and belladonna, of the same note, were procured also from Messrs. Leath and Ross. The above pilules of aconite and belladonna were obtained from Joseph Marples, homœopathic chemist, 23, Lece Street, Liverpool, and from Messrs. Thompson and Capper, homœopathic chemists, Liverpool and Birkenhead.

CORRESPONDENCE.

WE have to apologize to Dr. Sharp, of Rugby, for the inadvertent omission from the June *Practitioner* of a protest which he has made against some observations in an article by Dr. Ross ("Geometric Method in Medicine," Part III.) in the May *Practitioner*. Dr. Ross stated that Dr. Sharp's writings afforded no "principle of selection" as to cases in which the homœopathic doctrine of similars might be applicable as a basis of practice, and those in which it might not. Dr. Sharp's reply to this was a communication which we were unable to insert in the form in which it reached us; but we are anxious to give publicity to the fact that he repudiates the charge implied against his system, and points to various passages in his works which, he considers, completely disprove it. We should be sorry to fail in any courtesy to homœopathic practitioners simply because we reject their opinions, especially when, as in the case of Dr. Sharp, they are honest and original thinkers. Our reason for not admitting detailed replies to remarks on homœopathy which may appear in our pages is very simple, and had better be made here, once for all. It is this—that were we once to admit controversy on homœopathy into our journal, we should simply be swamped with materials, to an extent which nothing could justify, unless the subject were one of great importance. That, however, we cannot at all admit. We grant that homœopathy has an historical—an antiquarian—interest; and we think Dr. Ross has very well shown that it arose in a manner that was quite natural in the circumstances of the time. Certainly homœopathy was no more unreasonable than a vast number of other theories which were entertained by the most orthodox people: it was simply, in our opinion, one of the latest developments of a mode of medical thought which was necessarily doomed from the moment when physiology began to be scientifically applied to the practice of medicine.

We think it certain that physicians of the intellectual force of Dr. Sharp must sooner or later abandon homœopathy. But we deprecate the idea, that, because we cannot afford space for free controversy on homœopathy, we intend the slightest discourtesy to homœopaths.

We are sorry that, at the last moment, we have mislaid the paper containing Dr. Sharp's quotations of those passages in his works which, he considers, refute Dr. Ross's charge. If Dr. Sharp will kindly send us the references to the works and the pages in question, we will publish them with pleasure.

TAPPING FOR HYDATIDS OF THE LIVER DURING THE INVASION OF SCARLATINA.—I have just had a case in private practice which is interesting in more than one way. On May 28, Mr. Lewis, of Wingham, Kent, brought me a little girl, aged seven, who had a large tumour in the right hypochondriac region, and bulging the lower ribs. The first signs of the tumour—very slight—were noticed about eleven months previously. There was a history of a blow some little time before; and it was not quite certain that this might not have had something to do with the swelling. Matters progressed very slowly until about three weeks before the visit to me, when rapid enlargement had set in: the child had become very thin. When Mr. Lewis brought her to me, despite of her thinness, she had no such look as is associated with cachexia: she was rather flushed (with excitement, as I thought), and there was plenty of clear red colour in her face. On stripping her, the bulging of the lower half of the right ribs was very conspicuous; the liver appeared to extend from the fifth rib to $2\frac{1}{2}$ inches below the border of the ribs in the standing position. There was a more or less globular bulging, which pointed forwards and outwards. It seemed evident that this child had one of three things—either the blow (some twelve months before) had set up chronic suppurative inflammation; or there was a hydatid cyst (or cysts); or there was cancer. Looking to the great emaciation of the child, and especially to the rapid progress the tumour had lately made, I feared the latter was the most probable diagnosis; nor could I allow much opposing weight to the fact that there was no “cachectic” complexion; for I had just had a case of cancer (renal) in a little child where not the slightest impairment of natural colour took place till within four or five weeks of death. Nevertheless, it was impossible to be contented with ignorance as to the nature of the tumour, and I accordingly got Mr. Berkeley Hill to tap it *with the aspirator of Dieulafoy*. The child being placed erect, the liver was steadied from behind, and the trocar plunged in about one inch below the ribs, in the mammary line, where it was thought that some indistinct

feeling of fluctuation had been perceived. To our great pleasure clear aqueous fluid came out, and, with the aid of the aspirator, about four ounces were withdrawn. The fluid presented all the usual characters of that taken from hydatid cysts, except that we failed to detect hooklets with the microscope. Now comes the curious part of the story. The wound only gave a slight amount of pain for a few days, and the liver has decreased notably in size. But the very next morning after the operation a genuine scarlatina rash came out, so that the tapping must have been done during the actual invasion of the fever, though no one would have supposed that the child had any such thing upon her. The fever ran a mild course, and never in the slightest degree interfered with the favourable progress of the liver, which, there is every reason to hope, will go on to complete cure; the hydatid becoming encysted and the tumour gradually absorbed.

This case is only one of many which have recently accumulated, and which show the great value of the aspiratory trocar as a means of diagnosis, and the entire harmlessness of the operative procedure as performed with it. The child with (renal) cancer above referred to had a tumour exactly simulating, in external appearance, a hydatid tumour in the liver. A trocar (attached to the aspirator), being introduced into the most prominent position, only a teaspoonful of bloody fluid was extracted: under the microscope this showed an excess of white blood-corpuscles, and a number of other cells—free and contained in “mother-cells,” which obviously belonged to some rapidly growing solid tumour, which, under the circumstances, could hardly be referred to any other class than that of encephaloid cancer. The operation was quite harmless. Post-mortem examination verified the diagnosis as to the *nature* of the tumour, though its *seat* was found to be the kidney, instead of the liver, as had been supposed by me during the patient's life.¹—F. E. ANSTIE.

PHOSPHORUS CAPSULES.—In answer to a question from Mr. Samuel Burrows, Witheridge, North Devon, we beg to say, that the phosphorus capsules recommended by Dr. Broadbent are to be got at Corbyn and Co.'s, chemists, New Bond Street.

IMPORTANCE OF PHYSIOLOGICAL KNOWLEDGE IN EVERYDAY MEDICAL PRACTICE.—Our attention has been drawn to the discussion which took place, very lately, at the College of Physicians, respecting examinations for the minimum qualifications to practise medicine, and to the comments of sundry medical journals thereupon. We regret to say it is evident that the authorities

¹ My colleague, Dr. Allchin, with great shrewdness, had suspected the renal situation of the tumour.

who are arranging the scheme for a conjoint Examining Board have never taken the trouble to think seriously about the problems which are involved in the daily work of the general practitioner. They seem to suppose that to him physiology is a matter of trivial importance, while the mere distinguishing one disease from another, so as to be able to hang a label round the patient's neck, and punish him for being ill by giving a particular kind of physic, is everything. We hope to have an early opportunity of showing, in some detail, what a monstrous inversion of the facts is involved in this idea; but as the seniors of the College of Physicians have in their wisdom thought fit to ridicule the idea of the importance of accurate physiological knowledge to the practitioner who is absorbed in practical work, we shall confront them, at once, with a few plain questions. We ask, in the first place, whether a sound knowledge of the main divisions of food, the manner of their digestion, the kind of effect which particular morbid states have on the digestibility of particular aliments, and the difference between the digestive functions of children and of adults, is not of the very first importance? Secondly, whether the real nature, that is, the chemical composition, of the excreta, and their relations to the different sorts of work done in the body, is not an absolutely essential piece of knowledge for *any* practitioner? There are a score of similar queries which we could readily suggest, but we must leave this to a future occasion. As promoters, in our humble way, of the progress of therapeutics, we could not allow so barbarous a doctrine as that which is tacitly laid down in the Conjoint Examination scheme, as interpreted by the committee of reference, to pass unchallenged for a moment; and we hope that the profession at large will repudiate it with energy.

MINERAL WATERS OF FRANCE.—We have again to regret that an important paper from Dr. Leudet, on the waters of Eaux-Bonnes, so much employed in phthisis, has not reached us in time for publication in our present number; it is in the press, and will appear in August. Another French mineral water, that of Challes, near Chambéry, in Savoy, is now being investigated by Dr. Anstie at Westminster Hospital: the results will be published in due course. These waters at Challes have a great reputation in scrofulous diseases, and also in gout and gravel, and in skin diseases; they are rich in sulphide of sodium, carbonate of soda, &c.; with a dash of iodide of potassium. It remains to be seen whether the waters themselves, apart from the beautiful climate and scenery of Challes itself, are as directly therapeutic as has been supposed.

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Resorption urineuse et Urémie dans les Maladies des Voies urinaires. Contribution à l'Étude du Traitement de la Pierre dans la Vessie. Par le Dr. Jules Gerard. Paris: Baillière. Price 3s. 6d.

De l'Arthrite du Genou, et de l'Épanchement articulaire consécutifs aux Fractures du Fémur. Par le Dr. Paul Berger. Paris: Masson. Price 2s. 6d.

Étude sur les différentes Voies d'absorption des Médicaments. Par le Dr. A. L. Amagat. Paris: Baillière. Price 2s.

Du Vomissement: Contribution à l'Étude de l'Action des Vomitifs. Par M. le Dr. A. d'Ornellas. Paris: Henneyer.

Du Traitement de la Coqueluche par l'Hydrate de Chloral et par le Bromure de Potassium. Par le Dr. Pierre Armand. Paris: Delahaye. Price 1s. 6d.

Sull' Ultimo Stadio del Colera Asiatico, o Stadio di Morte apparente dei Colerosi; e sul Modo di farli risorgere. Per Professore F. Pacini. Florence. (Pamphlet.)

Neue Heilmethode bei Geschwüren. Von Prof. Dr. Nussbaum, München: Finsterlin.

Ueber das Wesen u. die Heilbarkeit der häufigsten Form progressiver Schwerhörigkeit. Von Dr. F. E. Weber-Liel. Berlin: Hirschwald.

¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

Department of Public Health.

THE MEDICAL OFFICER OF HEALTH.—HIS GENERAL DUTIES.

THE *general duties* of a Medical Officer of Health have never been more happily expressed than in an instructional minute issued by the General Board of Health in 1848. The minute in describing these duties needs but little modification to make it applicable to present needs, and it would have been well if some such minute had accompanied the Regulations of the Local Government Board as to the duties of a Medical Officer of Health. We quote that portion of the minute which relates to our present subject, making two or three necessary alterations within brackets. It runs thus:—

“ He will make himself familiar with the natural and acquired features of the place, with the social and previous sanitary state of its population, and with all its existing provisions for health, viz. with the levels, inclinations, soil [and humectation thereof, particularly in reference to level and variations of level of sub-soil water], wells, and water-springs [and other sources of water-supply] in the district [the quantity of the supply and the liability of the different sources to pollution]; with its meteorological peculiarities; with the distribution of its buildings and open spaces, paved or unpaved; of its burial-grounds and lay-stalls [lay-stall meaning here, it is to be presumed, a place for the deposit of animal ordure and other refuse]; with the plan of its drains, and sewers, and water-supply; with the nature of its manufacturing and other industrial establishments; with the home-accommodation of the poorer classes, and the facilities afforded them for bathing and washing; with the

arrangements for the burial of the dead ; and with the regulations in force for lodging-houses and slaughtering places ; for the cleansing of public ways and markets, and for the [deposit and] removal of [human ordure and other] domestic refuse. . . . He will take the best means in his power to become acquainted from week to week, and in times of severe disease from day to day, with the deaths and sicknesses in his district ; and he will inquire to what extent they have depended on removable causes. With the assistance of such subordinate officers as the Local Board may empower him to direct and superintend, he will without intermission see to the wholesomeness of his district ; taking care to bring its several parts under examination as often as their sanitary circumstances shall require ; and especially observing those places which have previously given occasion of complaint, or been subject to sickness. He will inquire as to the cleanly, wholesome, and weather-proof condition of houses ; as to their due ventilation and not overcrowded condition ; as to the efficiency of their drainage and water supply ; and as to the absence of dampness and offensive effluvia in and about them. He will examine from time to time the drinking-waters of the place [and their sources where a running stream], and will observe whether provisions are offered for sale in any damaged or adulterated state that is hurtful or illegal. He will occasionally visit all burial-places, and see whether they give any ground for complaint ; and he will habitually observe the slaughter-houses of the district, and other industrial establishments which are liable to emit offensive (especially animal) effluvia."

This is a goodly and, in appearance, somewhat formidable array of requirements. Yet, if we examine them narrowly, it will quickly become apparent that, perhaps with the exception of the observation of meteorological peculiarities, the different duties set forth naturally interlock with each other, and may reasonably be required from a medical man undertaking the duties of Medical Officer of Health. The requirements, indeed, refer almost solely to subjects presented ordinarily to the consideration of the Medical Officer of Health, and of which a competent knowledge is necessary in order to his advising as to the state of the population under his supervision in health

and sickness, and as to unwholesome conditions and their abatement.

The Medical Officer of Health who for the first time comes to the performance of his duties will be most at a loss in regard to works of reference for his guidance until he obtains the mastery of them. We propose, 'as 'the most useful mode of treating further this part of our subject, briefly to notice those works in the English language which have been published with this object or may be regarded from this point of view.

First among these is Dr. Parkes' "Manual of Practical Hygiene."¹ This book, although prepared specially for use in the medical service of the army, is simply unrivalled as a text-book of hygiene. Its special object is, in several aspects, as important to the Medical Officer of Health as to the army medical officer. Since the Crimean war, civil hygiene has been largely indebted to military hygiene for some of its most important advances. It is to the teaching of military hygiene, moreover, that we have chiefly to look for our knowledge of the temporary arrangements which have not unfrequently to be made for dealing with wide-spread epidemics among civil populations—epidemics in which temporary hospital accommodation, for example, is needed. Further, it is much to be desired that all Medical Officers of Health, in view of our volunteer forces, should familiarise themselves with the principles of military hygiene, with particular reference to camps and field service. There is, therefore, a peculiar advantage to the Medical Officer of Health in studying hygiene, from the particular point of view in which it has chiefly to be regarded in Dr. Parkes' work. And apart from this consideration, as a work on the principles and practice of hygiene in civil as well as military life, Dr. Parkes' book is without equal. It is the text-book of the Medical Officer of Health *par excellence*. A new edition of this work is announced.

An able and very useful Handbook of Hygiene² has recently

¹ A Manual of Practical Hygiene, prepared especially for use in the Medical Service of the Army. By Edmund A. Parkes, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School. 3rd edition. London: J. Churchill and Sons, 1869. 8vo. pp. 640.

² A Handbook of Hygiene. By George Wilson, M.A., M.D. Edin., Medical Officer, H.M. Convict Prison, Portsmouth. London: J. and A. Churchill, 1873. Sm. 8vo. pp. 380.

been published by Dr. Wilson, the medical officer of H.M. Convict Prison, Portsmouth. This work, in addition to the practical details of hygiene, discusses briefly some of the more general questions relating to physical and moral deteriorations. It has sections on "hereditary influence" and the "causes of deterioration and disease." One noteworthy merit of this "Manual" is the carefulness with which the author indicates the sources from which he has obtained information, and where more detailed information may be obtained on the subjects he treats than is consistent with his plan. Each chapter, in fact, is not merely a summary of existing knowledge, but an index to trustworthy sources of that knowledge. The book has a thoughtful and suggestive chapter on the duties of a Medical Officer of Health, and in an appendix is given an abstract of, with references to, the principal sanitary laws.

A work which has just been published by Dr. Edward Smith, entitled a "Manual for Medical Officers of Health,"¹ would on the first aspect seem to set aside the necessity for our dealing with this subject at all. Dr. Smith was formerly the Medical Officer of the Poor-law Board, and now, since the formation of the Local Government Board, he holds the double position of inspector and assistant medical officer for poor-law purposes of that Board. The work is published by the publishers of the Local Government Board, and it has been largely advertised by them as "a complete guide for sanitary authorities and their officers." No claim of this kind, it is right to state, is set forth in the book itself; and after a perusal of it, it is difficult to understand how such a claim can have been systematically advertised. The book derives its importance from the official position of the author, and also, to some extent, the official position of the publishers; and although it is not published as an official work, it is difficult to avoid attaching to it a quasi-official character. The work takes the form of a commentary upon the different duties of a Medical Officer of Health as defined in the Orders of the Local Government Board of the 11th November, 1872, and it professes to include the various provisions of the Sanitary Acts bearing upon those duties. The

¹ Manual for Medical Officers of Health. By Edward Smith, M.B., LL.B. Lond., F.R.S. Knight and Co., 1873. Sm. 8vo. pp. 339.

first chapter is devoted to the "Causes and Distribution of Disease," and the first section of this chapter refers to "the construction of houses in reference to the production of disease." This section consists of twenty-one pages, of which not less than eleven and a half are occupied with quotations from the Metropolitan Buildings Act, 1855, and the Towns Improvement Causes Act, 1847, both of which Acts have no bearing whatever on the regulations of the Local Government Board. Moreover, the provisions of the Sanitary Acts, under which the Medical Officer of Health and his Sanitary Authorities appointed under the Public Health Act, 1872, must work, and the suggestions for by-laws relating to buildings, issued first under the authority of the Home Secretary and now of the Local Government Board, and upon which the by-laws of most of the urban sanitary districts have been framed, are not noticed! The second section is devoted to "ventilation." Here again the provision as to ventilation of public buildings in the Towns Improvement Act, 1847, is absurdly quoted; while the provisions made under by-laws both as to private and public buildings under the Local Government Act, 1858, which immediately concern the Medical Officers of Health, are altogether ignored. Again, it is noteworthy here that the only practical data given by Dr. Smith as to cubic space and floor space are those relating to work-houses, and which, as concerns metropolitan and other workhouse schools, are those under which the extraordinary persistency and spread of purulent ophthalmia have lately occurred. The third section deals with "drains and drainage; gutters, and other accumulations of filth." Here again, with the exception of a solitary section from the Common Lodging Houses Act, 1851, Dr. Smith is quite at sea as to the provisions of the Sanitary Acts and the official documents of the Local Government Board on the subject. The fourth section deals with the "personal habits of the people." Of the twelve and a half pages which this section fills, all but *one page and a sixth* are occupied with extracts from the Contagious Diseases Act, 1866, with which the Medical Officer of Health has no concern whatever! "Poverty" and "climate and topographical influences" are the subjects of sections five and six; the former subject being held to be sufficiently treated in a page and a quarter, the latter in seven

pages, of which two, containing statistical tables by Dr. Gairdner and the Registrar-General, have no bearing on the subject. Section 7 refers to "inquiries and systematic inspection," and is rather an expression of opinion as to the meaning of the Local Government Board's Regulations under these heads than a practical suggestive discussion of them, based either upon personal experience of such inquiries or the experience of others. "Let us assume," writes Dr. Smith, "that the kind of disease and the number of persons afflicted with it may by proper inquiry be ascertained, still the essence of the direction is the connection which may exist between them and certain supposed causes of disease in the same area. The former may be based upon facts and produce conviction, whilst the latter is very likely to be speculative, and, being based more upon opinions than facts, may not carry conviction," and so forth.

Chapter II. of Dr. Smith's "Manual" relates to the "Personal Action" of the Medical Officer of Health "under the Diseases Prevention Act, 1855, and during an attack of epidemic disease." Dr. Smith, properly treating the latter part of the heading first, considers that personal action in relation to epidemic disease relates to the approach to or contact with infected persons or things; impure water; decomposing animal and vegetable substances, and filthy homes; sewage emanations; impure air; and meteorological and seasonal influences. To us there is here a curious confusion of "the conditions injurious to health" provided for in another regulation, with the action required from a Medical Officer of Health where epidemic disease is present and spreading. With the former conditions the Medical Officer of Health is expected to deal continuously; the duties he has to perform during an attack of epidemic disease are occasional and special, and, as far as they touch upon the unwholesome conditions referred to, have particular bearings of which Dr. Smith makes no note. The mode in which Dr. Smith deals with the different conditions he enumerates, betrays throughout the confusion indicated by his mode of placing them. As to infection, he thinks it sufficient to quote the provisions of the Sanitary Act, 1866, Common Lodging Houses Act, 1851, and Nuisance Removal Act, 1860, but omits the important provision of the Public Health Act, 1872! He cites also the Orders in Council as to

cholera, of the 29th July and 3rd and 5th August, 1871; but gives none of the important information, of such great practical importance to the Medical Officers of port Sanitary Authorities, of the mode in which these Orders have been carried out. As to *impure water*, he first quotes various provisions of the Sanitary Acts, and then actually gives *nine* pages of quotations from the Metropolis Water Act, 1871, with the provisions of which neither the Medical Officers of Health nor the Sanitary Authorities under the Public Health Act, 1872, have anything whatever to do. Dr. Smith may say that he intends his Manual for all Medical Officers of Health, metropolitan and provincial. But in that case the omissions become more extraordinary still. For he makes no reference to the cardinal provision, as concerns the Metropolitan Medical Officer of Health, of the Metropolitan Waters Acts, viz. filtration, and recent legislation to secure this. He omits also and makes no reference to the "Regulations" issued under the Act he quotes, although a most necessary part of it. He next, under the head of "Examination of Drinking Waters," follows almost entirely Messrs. Wanklyn and Chapman, quoting all important details largely from their well-known work. The bulk of the remainder that he has to say is taken crudely from the Rivers Pollution Commission; and Dr. Smith actually interpolates, as if in the midst of a discussion of potable waters, the Rivers Pollution Commission's *suggested standards of purity to be required before sewage, manufacturing or other liquid refuse*, be allowed to flow into any river or stream—standards of which the Commission says, their great mildness is illustrated by the fact that the ordinary condition of the waters of the *notoriously polluted rivers Clyde and Irwell, below Glasgow and Manchester respectively, would scarcely infringe any one of them!* Dr. Smith interpolates these standards in this chapter on drinking-water and his discussion of potable waters, with no other statement than that the Commission considers "any liquid as unfit to enter a stream" which comes within them. Very few without previous knowledge of the subject would read this part of Dr. Smith's work without coming to the conclusion that these standards were a guide to the potability of waters. Need we go further in this most distasteful analysis? The succeeding chapters have like defects in law and detail as the preceding. It

may be noted, however, that in the chapter on "Routine, Attendance, and Reports," with the exception of a brief reference to some private efforts of registration of diseases, the suggestions offered are founded upon Poor-law classification and returns of sickness.

From beginning to end of Dr. Smith's book the important suggestions and memoranda of the Engineering Department of the Local Government Board, the even more important memoranda and reports of the Medical Department of the Board, and the classification and reports of the Registrar-General, all official materials essential for a Medical Officer of Health to become acquainted with, are absolutely ignored. In fact Dr. Smith, in this so-called "Manual," sets aside entirely, with the briefest reference to certain work done by himself and by Dr. Buchanan, and the quotation of a report by Mr. Arnold Taylor, all the important sanitary work done by the Government Medical and Engineering Departments during the last thirteen years; the reports and official memoranda on which have formed up to the present time the necessary basis of official sanitary work, whether central or local. We state the facts without further comment.

Of hardly less importance, he draws no lessons from the numerous invaluable reports of the metropolitan and provincial officers of health. For Dr. Smith, neither a Liddle, nor a Duncan, nor a Trench seems to have existed!

The great mine of information on public hygiene in this country is formed by the thirteen annual reports of the Medical Officer of the Privy Council (now of the Local Government Board), and the different local reports of the Medical Inspectors of the Board. Unfortunately, some of the annual reports are out of print, and the local reports are not easily to be obtained. Each of Mr. Simon's annual reports is, however, full of information of the greatest importance to Medical Officers of Health, and all should be closely studied by them whenever practicable. And in reference to this part of our subject, it is desirable to refer to the work of Dr. Seaton, the Assistant Medical Officer of the Local Government Board, on Vaccination.¹ The Medical Officer of Health has no official responsibility in respect to carrying out vaccination, but it is necessary that he should make himself master of the question

¹ A Handbook of Vaccination. By Edward C. Seaton, M.D. Macmillan and Co.

in its public relations. For this purpose, and as a text-book on the whole subject, Dr. Seaton's work is unsurpassed.

The different reports of the Registrar-General furnish information essential for the Medical Officer of Health. He should study the weekly reports (the bills of mortality for the metropolis) to guide him as to his own weekly reports. The quarterly reports give him the data which will enable him to judge with some degree of accuracy the state of health of his own district during the corresponding periods. The annual reports will teach him what statistical reports should aim at. It is one of the distinguishing merits of Dr. Parkes' Manual that it contains instruction on vital statistics.

The Medical Officer of Health will probably find most difficulty in dealing with that portion of his duties which relates to nuisances from trades. There is no text-book of noxious trades, and information regarding them has chiefly to be sought in blue-books. The action of the Sanitary Authority in regard to noxious trades is, as a rule, one of considerable difficulty, and the Medical Officer of Health, in advising his authority, should have a clear knowledge as to the substantial bearing of the law. Indeed, not only in this matter, but in all matters, the Medical Officer of Health should strive to obtain a competent knowledge of the law under which he is acting. For this purpose the best work of reference is Mr. W. Cunningham Glen's "Public Health and Local Government Laws."¹ With a knowledge of the Law of Nuisance in respect to trades, and a careful study of trades coming under his cognizance, a Medical Officer of Health should experience no insuperable difficulty in advising his Sanitary Authority. As a text-book of trades, Dr. Ure's "Dictionary of Arts, Manufactures, and Mines" (Longmans), may be referred to.

THE ETIOLOGY OF CONSUMPTION.

DR. BOWDITCH, following the same method of inquiry which led to such excellent results in his hands as to the relations of soil-moisture to consumption, has, for the State Board of Health of Massachusetts, been pursuing an investigation con-

cerning the Antecedents of Consumption in that State. The results of this investigation are given in the recently-published Fourth Annual Report of the Board, and are of considerable interest, as showing the "medical public opinion," as Dr. Bowditch phrases it, on the subject. The method of inquiry adopted was that of a circular letter, containing a series of questions addressed by the State Board of Health to 210 medical practitioners in the State. The value of such an inquiry is chiefly from the indications which it gives of more precise lines of investigation. Dr. Bowditch, in his report of the results of the inquiry, confines himself almost entirely to a numerical statement of the answers to the different questions, and the reproduction of detailed portions of the correspondents' replies relating to them; but he expresses a hope, at a future time, to give more particularly his own views on the possible "Prevention of Consumption in Massachusetts." In the following analysis of the results of the inquiry, the questions are noticed in the order of affirmative opinion.

1. *Hereditary Influence*.—Two hundred and five of the 210 physicians believed in the great importance of hereditary influence in causing or promoting consumption; one only denied the importance; and four did not reply.

2. *Wet Location*.—One hundred and sixty-nine (80 per cent.) held that a wet location favoured the development of the disease; 21 (10 per cent.) denied this; 2 (0.95 per cent.) were doubtful; and 19 (9.04 per cent.) returned no answer. On the near unanimity of the correspondents on this question, Dr. Bowditch observes: "This is probably owing to the fact that investigations carried on in Massachusetts many years since by myself, and subsequently in England by Dr. Buchanan, under the special directions of the Medical Officer of the Privy Council, have fully proved that residence on a damp soil tends to the production and promotion of consumption in New and Old England."

3. *Overwork in Trades*.—One hundred and seventy-one (80.14 per cent.) believed that overwork in trades had a part in causing consumption; 8 (3.8 per cent.) disbelieved; 7 (3.33 per cent.) were doubtful; and 24 (11.42 per cent.) gave no answer.

4. *Special Trades*.—One hundred and fifty-eight (75·24 per cent.) affirmed that certain trades fostered consumption; 9 (4·28 per cent.) denied the influence; the same number were doubtful; and 34 (16·19 per cent.) did not answer. The special trades enumerated by the correspondents were chiefly dusty trades, and trades followed in ill-ventilated and overheated mills and workshops.

5. *Overwork of any kind*.—One hundred and fifty-two (72·38 per cent.) affirmed overwork as a cause; 13 (6·19 per cent.) held the contrary; 7 (3·33 per cent.) were doubtful; and 38 (18·09 per cent.) did not answer.

6. *Overwork at Study or College*.—One hundred and fifty-three believed in over-study as a cause; 21 (10 per cent.) did not believe this to be a cause; 10 (4·76 per cent.) were doubtful; and 26 (12·38 per cent.) did not answer.

7. *Inordinate Sexual Indulgence*.—One hundred and fifty-two (72·38 per cent.) aver this as a cause; 16 (7·61 per cent.) denied this; 11 (5·23 per cent.) were doubtful; and 31 (14·76 per cent.) did not answer.

8. *Mental Trouble*.—One hundred and fifty (71·42 per cent.) state this as a cause; 18 (8·05 per cent.) answered in the negative; 10 (4·76 per cent.) were doubtful; and 32 (15·23 per cent.) gave no reply.

9. *Exposed Location or Residence*.—One hundred and ten (61·42 per cent.) held that exposure of residence might have some effect in producing the disease; 38 (18·09 per cent.) believed that it had no such effect; 7 (3·33 per cent.) were doubtful; and 36 (17·14 per cent.) did not answer.

10. *Drunkenness*.—(a) *In the Individual*. One hundred and nine (51·90 per cent.) decidedly believed in this as causing or promoting the disease, and 8 held it to be a secondary cause; 47 (22·38 per cent.) did not believe so; 13 (6·19 per cent.) were doubtful; and 33 (15·71 per cent.) gave no reply. (b) *In Parents*. One hundred (58·13 per cent.) of the answers were in the affirmative; 44 were in the negative; 19 were doubtful; and 38 physicians did not reply.

11. *Severe Bodily Injuries*.—Affirmative, 102 + 6 with a qualification (51·42 per cent.); negative, 38 (18·09 per cent.); doubtful, 10 (4·76 per cent.); no reply, 54 (25·71 per cent.).

12. *The restraining influence of Child-bearing.*—One hundred and forty-six (69·52 per cent.) believed that consumption is checked by child-bearing; 37 (17·61 per cent.) expressed the opinion that, although consumption was checked by child-bearing, it ran a more rapid course after delivery; 33 held that child-bearing exercised no influence over the progress of the disease; 7 were doubtful; and 24 gave no answer.

13. *Contagion or Infection.*—One hundred and ten (52·38 per cent.) believed in the contagiousness or infectiousness of consumption; 45 (21·42 per cent.) denied this property; 27 (12·85 per cent.) were doubtful; and 28 (13·33 per cent.) gave no reply.

14. *Marriage.*—Eighty (39·52 per cent.) believed that marriage under certain circumstances promoted the disease; 63 (30 per cent.) held the opposite opinion; 12 were doubtful; and 52 (25·66 per cent.) gave no answer to the question.

15. *Total Abstinence.*—Twenty-six (12·38 per cent.) considered total abstinence a cause of consumption; 106 (50·4 per cent.) did not regard it as a cause; 20 were doubtful; and 58 (27·62 per cent.) declined to answer the question.

Other questions related to the *Prevention of Consumption.*

16. *Prevention in Children hereditarily predisposed.*—One hundred and twenty (57·14 per cent.) believed that consumption can be prevented in those children who are hereditarily predisposed; 15 (7·14 per cent.) held that it could be retarded; 20 (9·52 per cent.) held that there was no prevention; 10 were doubtful; and 45 gave no answer.

17. *Means of Prevention in Children hereditarily predisposed.*—Twenty-four held that this was practicable by general hygienic measures; 96 advocate certain specific directions more or less elaborately described by them; 12 averred that there were no means of prevention known; 5 gave doubtful replies; and 73 gave no answer to the question.

18. *Is a Drunkard less liable to Consumption?*—The answers in the negative were 113 (53·80 per cent.), in the affirmative 27 (12·86 per cent.); 7 (3·10 per cent.) held drunkenness to retard the disease; 17 (8·09 per cent.) were doubtful; and 46 (21·90 per cent.) did not answer it.

19. *Will Total Abstinence save a man from Consumption?*—Thirty-eight (18·09 per cent.) answered in the affirmative; 89

(42·38 per cent.) in the negative; 22 (10·0 per cent.) were doubtful; and 56 (26·67 per cent.) gave no reply.

It will be observed in the foregoing analysis that the causes of consumption most generally accepted by physicians practising in Massachusetts are, first, hereditary influence (98 per cent. of opinions expressed affirmative); next, a wet location of dwelling (80 per cent. affirmative opinions); third, trade injuries, either overwork in trade or special trades; and lastly, together, over-studying, mental trouble, and sexual indulgence. About half the correspondents believe in drunkenness of parents or of the individual, and severe personal injuries, being causes of consumption; and that contagion or infection plays a part in its causation.

The report includes a great many interesting observations, such as probably would hardly be possible in England, as to particular influences in particular families and persons. Incidentally, it is an interesting question respecting the believed small mortality among Jews, and a suggested connection of this asserted fact and the non-use of pork. Dr. Bowditch gives the following statements on the authority of Rabbi Dr. Guinzburg, of Boston, U.S. The Jews living in Boston are about 5,000. There have not died of consumption, during the last five years, more than 8 or 10 Jews in the various congregations. If these data be correct, Dr. Bowditch states, the proportion of deaths in Boston from consumption, to 100,000 living, during the period in question, was, among all religions, 338; among Jews, 40.

ON THE CONCURRENCE OF EPIDEMICS.

THE arithmetical study of disease has in these latter years fallen very considerably into disuse, at least in this country. Dr. Buchanan, in a recent paper read before the Epidemiological Society, has sought to revive it in respect to the concurrence of epidemic diseases, with the result of removing

¹ The Law relating to Public Health and Local Government, including the Law relating to the Removal of Nuisances, Injuries to Health, the Prevention of Nuisances, &c. With Supplement on the Public Health Act, 1872. By W. Cunningham Glen, Barrister-at-Law.

much haziness of thought on some of the various questions relating thereto. Hitherto, he observed, attempts to investigate the laws under which epidemics prevail together, or refuse to prevail together, have been based on investigations in particular towns, or localities; and a number of much disturbing, yet imperfectly understood, elements in the case have always been in the way. We have not known what allowance to make for the difference of the so-called cycles of one disease to those of another, nor for the tendency of one disease to attack one part of the community rather than another, nor for the comparative facility with which each of these diseases may be imparted, nor for the different ways in which they spread, nor for the different seasons at which they tend to occur. Moreover, we have had, until lately, few data about epidemics, except such as related to yearly periods, and a year is too long a time for the purpose of the investigation. And since quarterly returns of the Registrar-General, the only extended data respecting epidemics in short periods, began to be published, only eleven quarters have elapsed—a time very much too short for the investigation of epidemic concurrences in any particular town or locality. Dr. Buchanan, instead of regarding the same place throughout a series of epidemic periods, proposed to regard a number of places during the same epidemic period. He proposed to investigate the concurrences of epidemics observed in these places during one quarter of a year, and to compare the observed concurrences with the concurrences that would, on mere arithmetical grounds of chance, be calculated to occur. With this object, and mainly as illustrative of the method of investigation, he examined the 92nd Quarterly Return of the Registrar-General—that for the three months, October, November, and December 1871—and studied the distribution and combinations of the seven epidemic diseases there tabulated.

On opening this, or any other Quarterly Return, Dr. Buchanan observed, and looking down the column of epidemics, the first thing that strikes the student is how seldom two or more epidemics come together in one district as compared with the frequent occurrence of single epidemics in a district. Thus, omitting the metropolis and the larger districts (for the present reason that these figures are not at once understood by the eye),

perhaps not more than one place with two epidemics will be noticed for ten that have one epidemic. A very little reflection shows that, unless there be some reason independent of chance, this smaller frequency of pairs than of single epidemics must happen as a mere matter of arithmetical probability. Thus, if epidemic small-pox occurs in one-tenth of the districts, and epidemic measles also occurs in a tenth of the districts, then, if there be no other element than chance at work, we should not expect to find small-pox and measles together occurring in the same number, one-tenth, of the districts. On the contrary, as measles occur in one-tenth of all the districts, assuming still there is nothing but chance in its distribution, it would probably be found in one-tenth of any particular group of districts. It would therefore be probably found in one-tenth of that group of districts which had small-pox, and the group which had small-pox is one-tenth of the whole number of districts. The number of districts which we should expect to have measles and small-pox at one and the same time, then, is, upon these data, one-tenth of one-tenth of the whole, or one-hundredth of the whole. In arithmetical terms, if the number of districts be n , if the number of these districts having epidemic small-pox be p , and if the number of these districts having epidemic measles be q , then the fraction $\frac{pq}{n}$ represents the number of districts in which small-pox and measles are to be expected to concur. This is where no element but chance is at work. But if there be in the background any common condition assisting in the production of small-pox which also assists in the production of measles, we should expect to find the frequency of actual concurrence between these two diseases greater than that calculated as the mere arithmetical chances of their concurrence. If, on the other hand, there were in the background some antagonism between the conditions under which small-pox was produced and the conditions under which measles was produced, we should expect to find the frequency of actual concurrence between the two diseases less than that calculated as the arithmetical chance of their concurrence. In either case, observed Dr. Buchanan, it is clear that before any inquiry of a statistical kind can be made respecting community of causation or mutual

antagonism, the arithmetical probability of their concurring, as a mere matter of chance, must be determined ; and from this arithmetical probability as a standpoint, further statistical inquiry as to matters that may be in the background must proceed.

The advantages which attach to the method of inquiry pursued by Dr. Buchanan he states as follows :—Whatever assumption is made in examining the prevalence of one disease is carried on into the examination of the concurrence of diseases. Thus, whatever may have been the so-called epidemic constitution of the period, whether disposing to scarlet fever, or to measles, or to hooping-cough, matters nothing. Whatever may be the cyclical period of each disease, again, is of no consequence, for here we examine them all at once ; whatever tendency there may be for one epidemic to occur among different classes of a community or at different seasons, is again of no consequence ; we accept the tendency as comprised in the facts of the several single prevalences of epidemics, and accept it equally when we apply those facts to reckon the probabilities of concurrence at the same period. So, again, how each disease has come to be epidemic has no bearing on the method of inquiry, as any assumption on this point concerning single diseases is carried forward in considering their combinations. It does not even matter what definition we assume of an "epidemic" ; for, whatever the definition, we reckon the probability of combinations occurring upon the same assumption that we make in noting the epidemic prevalence of each disease singly.

Dr. Buchanan next referred to the question as it relates to larger communities, but as this does not affect the conclusions to be derived from the data he made use of, we pass this portion of his paper over. In illustration of what has gone before, Dr. Buchanan took his data from extra-metropolitan registration districts having populations below 10,000 a-piece. These districts (*sub-districts* of the Registrar-General) number 1,490 out of a total of 2,195 districts in England and Wales ; about two-thirds of the whole. In the last three months of 1871 the following epidemics occurred in them, and these epidemics happened singly or in combination in the following numbers :—

Occurring	Small-pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhoea and Cholera.
In 478 districts, singly . . .	68	24	104	18	53	112	99
In 164 districts, along with one other epidemic . . .	35	36	62	9	44	77	65
In thirty-seven districts, along with two other epidemics . . .	13	12	23	3	14	26	20
In six districts, along with three other epidemics . . .	4	3	2	2	3	4	6
In one district, in a combination of five epidemics . . .	1	1	1	—	—	1	1
Making a total of epidemic occurrences . . .	121	76	192	32	114	220	191

Upon this data the probability of concurrence of any two of these epidemic diseases in the 1,490 districts can be calculated. The number of small-pox occurrences was 121 in the 1,490 districts; the number of measles occurrences was 76; then the number of probable concurrences of the two together, all other considerations but the arithmetical one of chance being set aside, is, as we have seen, $\frac{121 \times 76}{1,490}$, or 6.1. In six out of the districts, therefore, we should expect to find measles and small-pox epidemic together. Now, the seven above-mentioned diseases can be paired in twenty-one different ways; and reckoning out, in the same manner, the number of probable concurrences of each pair of epidemics in the three months in the 1,490 districts, we obtain the figures in column A. Side by side with the numbers indicating the probable concurrences of the several pairs of epidemics, I have placed in the table a column, B, showing the actual number of concurrences that did actually occur in the three months in the 1,490 districts examined. It will be seen that every possible pairing of epidemics, all twenty-one of such pairs, did actually happen in this period; and it will be seen that there is a good deal of general correspondence between the calculated and the observed

figures. But on looking at particular pairs we find departures, of more or less magnitude, of the actual from the calculated degree of concurrence. More or less of such departures are inevitable in any limited numbers of events, and we have only had 1,490 occurrences with which to deal. Investigation on the present plan can, however, without difficulty be widely extended; and when, by such extended investigation, the true similarity or dissimilarity between the calculated and observed figures has been established, the point is reached at which interest for the student of epidemiology and of pathology begins; and it is this point that Dr. Buchanan sought to indicate in his paper.

PAIRS, Combination of.	A. Calculated number of concurrences.	B. Observed number of concurrences.
Small-pox and Measles	6.1	8
" " Scarlet Fever	15.4	14
" " Diphtheria	2.6	1
" " Hooping-cough	9.2	15
" " Fever	17.8	24
" " Diarrhoea	15.5	15
Measles and Scarlet Fever	9.7	15
" " Diphtheria	1.6	3
" " Hooping-cough	5.8	11
" " Fever	11.1	20
" " Diarrhoea	9.6	16
Scarlet Fever and Diphtheria	4.1	5
" " Hooping-cough	14.6	10
" " Fever	28.1	44
" " Diarrhoea	23.3	30
Diphtheria and Hooping-cough	2.4	2
" " Fever	4.7	4
" " Diarrhoea	4.1	6
Hooping-cough and Fever	16.7	18
" " Diarrhoea	14.5	25
Fever and Diarrhoea	28.0	35
Total number of concurrences	250.0	321

Dr. Buchanan, noticing the possible significance of certain of the foregoing numerical results, referred to the excessive frequency of the concurrence of small-pox with other epidemics, of diarrhoea with all other epidemics except small-pox, of scarlet fever with the "fever" of the Registrar-General; and on the other hand, the absence of any notable excess of frequency of concurrence between scarlet fever and diphtheria, or between scarlet

fever and hooping-cough; and in regard to these results he expressed the opinion that they might be followed up with the help of more numerous data, and perhaps by the aid of other methods of inquiry, until they yield results of real importance.

Dr. Buchanan proceeded to discuss the question he had proposed to examine by a different standard with substantially the same results.

NOTE ON THE HEALTH ASPECTS OF SEWAGE IRRIGATION.

WE have been favoured by Dr. R. Boyd with the following note on this subject:—

"The following statement of sewage irrigation, extending over a period of twenty years' residence and upwards in the Somersetshire County Asylum, may perhaps be considered of sufficient interest for insertion in the *Practitioner*, *à propos* of the article in the May number.

"The architect of the Somerset Asylum, the late Mr. Moffat, who was specially interested in sewage arrangements in London and its neighbourhood, took advantage of its sloping site and had a large brick tank built; it was situated 141 yards distant from the front of the house, and the sewage was carried into it by drains. About twelve acres of land adjoining and below this tank were irrigated with the sewage. Trunk drains were made through the land, with hatches at certain distances, and these drains were again carried into smaller tanks, from which the sewage could be pumped or drawn as required. The high road ran through part of the land, and a pipe was laid underneath by which the sewage was again carried into meadow land on the other side of the road. The crops thus produced were most exuberant, especially cabbages, of which some were five feet in diameter and from fifty to seventy pounds in weight; in one season Italian rye-grass was cut seven times, so rapid was its growth. These crops were found to increase the quantity and improve the quality of the cows' milk.

"The food was varied, and it was ascertained that the quantity consumed by one cow, on four days respectively, was 150 lbs. of cabbages the first day, 160 lbs. of turnips the second day, 40 lbs. of hay the third day, and 155 lbs. of mangold-wurzel the fourth day.

"A double-barrel liquid manure lifting pump with iron pipes was subsequently provided, by which the sewage was raised to small tanks in the garden behind the house; this sewage was found especially useful in dry seasons. During my residence no case of enteric fever ever occurred, or no epidemic or disease of any kind which could be attributed to the sewage irrigation. The vegetables and milk thus produced were all consumed in the establishment. On the greater part of the land under sewage, cultivation by spade labour was employed, and no complaint was made by the labourers, attendants, or patients employed under

them, of whom the number was very great. The sewage became deodorised when it came in contact with the soil, which was a red loam on clay.

"The sewage of the farm-yard was also collected in a tank and was carried by means of a water-cart and distributed on the grass land : this was occasionally offensive at the moment, when the weather was dry and earth hard. There was an escape to the high road from the overflow of the tank when neglected : this was complained of, and upon one occasion the occurrence of malignant scarlatina in a family was attributed to this cause ; it was subsequently ascertained that the well which supplied their house was close to and below the level of the churchyard."

1. Henry G.

2. Henry Miller

3. Henry Miller

21 Oct 1872.

4. Henry Miller

Henry Muller
14/12/72
173 Manor St. Clapham

The weather is fine but cold
Henry Muller
24 March 1972
Written while resting my wrist on
the desk

THE PRACTITIONER.

AUGUST, 1873.

Original Communications.

THE VALUE OF SODIC BROMIDE AS A NERVOUS SEDATIVE.

BY W. AINSLIE HOLLIS, M.D.

SODIC bromide, or, to use its vulgar name, bromide of sodium, although for some years known to have a therapeutic action somewhat similar to its analogue, the potassium salt, has, I consider, been lately somewhat unfairly "shelved" by pharmacutists, whilst its more highly favoured rival has been introduced to the profession as *the remedy* for nearly every ailment flesh is heir to. Of late years, however, Dr. Richardson (in the pages of this journal and elsewhere) has shown the value of other combinations of bromine than the potassic salt alluded to; and Hammond, Nunneley, Lavallée, and others have also investigated the action of the alkaline salts of bromine. The last gentleman has observed the action of the sodic salt in the treatment of spermatorrhœa.¹ Hammond showed that the calcium salt was of great use in allaying nervous excitement arising from excessive mental anxiety, and that it induced sleep when the potassic bromide in large doses failed to do so.² I need not here allude

¹ "Bromures alcalins dans le Traitement des Affections génito-urinaires," noticed in "Annuaire de Thérapeutique pour 1873," par A. Bouchardat. Paris † p. 230.

² *New York Medical Journal*, 1871, II. p. 594.

to Dr. Richardson's well-known observations on the organic bromides, as they have already been published in these pages.¹

I have lately had an opportunity of trying the effect of the sodium salt in various forms of nervous disease, and with results which at all events, I think, justify a more full and complete investigation of its properties before finally assigning it to that limbo of useless drugs which has assumed of late years such unwieldy proportions.

The salt has a pungent saline flavour, is freely soluble in water, and forms a colourless solution. During the course of one evening I took five-and-twenty grains of the bromide upon an empty stomach, in two doses of ten and one of five grains, at intervals of about half an hour. The effect on the pulse and temperature was negative; upon retiring to rest as usual, I experienced a somewhat pleasant drowsy sensation and slept remarkably well, although I awoke several times during the night, as is customary with me. Towards morning, however, I felt a numbness and tingling in both fore-arms and in my right leg: these sensations did not entirely disappear for some hours subsequently. During the operation of the drug, I passed the usual quantity of urine, of normal colour. I ought to state that shortly after each dose of the medicine I experienced a burning sensation at the epigastrium, which quickly passed off. Beyond the above symptoms—pointing principally to a “decreased sensibility of the nerves which govern common sensibility,” to use Dr. Richardson's words—of the bromides generally, no special peculiarity was observable in the action of the sodic bromide.

I shall here briefly give my experience of its effects as a remedy in various forms of nervous excitement.

Fred. B. came under my treatment on Nov. 13, 1872, for confirmed epilepsy. He was twenty-one years of age, of a dark sallow complexion; his face was spotted with *acne punctata*; his eyes somewhat deep-set, and placed closer together than usual. From his early childhood he had been subject to inveterate epileptic attacks, the fits averaging in frequency three or four weekly. For the first month he was treated with half-drachm doses of the bromide of potassium three times daily, combined with a nightly dose of one-third of a grain of the

¹ *Practitioner*, June 1871.

extract of belladonna. As he still continued to have severe fits occasionally (although their frequency was somewhat diminished), a seton was inserted in the back of his neck. For a short time subsequently the fits appeared to diminish, both in severity and frequency, under this treatment: he, however, complained of lowness of spirits and general debility. I accordingly, on the 19th December, prescribed a grain of sulphate of iron, and the same quantity of sulphate of zinc, to be taken three times daily. This treatment was only continued a week, as the symptoms returned with all their former severity. I then gave him forty grains of the potassic bromide three times during the twenty-four hours: he, however, had three severe fits in the following week. On January 30, 1873, I added half a drachm of the succus conii to the mixture: the fits, although somewhat diminished in frequency (one occurring once a week upon the average), were severe when they took place. With the exception of a fresh seton inserted into the neck on March 13, no change of treatment was made until May 22. At this period, as the epilepsy continued, I tried the effect of a small dose (three grains) of sodic bromide in water three times daily: during the next week he had two fits. On May 29 I increased each dose to fifteen grains: during the following week he had not had a fit, although he stated that he occasionally "fell about." I consider this was probably the result of attacks of the "petit mal;" in other words, the greater evil was transformed to the less. On June 5 I augmented the dose to twenty grains of the salt. The last time I saw him he stated that he had had one fit, and that his general health was much improved. I have detailed this case somewhat fully in order to show that in the sodic bromide I consider we have a powerful remedy in arresting (or at all events diminishing) attacks of epilepsy. The sodium salt appeared in the above case to exert a greater proportional influence in checking the epileptic seizures than did the potassium salt.

In another case, a boy of fourteen years of age, who had been subject to epilepsy since birth, after acquiring the habit of taking ten grains of the potassic bromide in decoction of bark three times daily, with the effect of checking the seizures, was placed upon three-grain doses of the sodium salt in place of other

medicine; and even this small quantity prevented the return of the fits, although he felt much depressed. This depression of spirits I find is a very frequent accompaniment of the exhibition of this salt, and I ascribe it partly to the fact that in all the cases I have or shall have occasion to detail in reference to the action of sodium salt, the watery solution of the drug was invariably used. I have no doubt that any such depression of spirits might be advantageously counteracted by the addition of some tonic, such as cinchona bark, to the daily dose of the bromide, or possibly by the admixture of small quantities of the tincture of Indian hemp, as has been found useful upon the accession of similar symptoms after the exhibition of the potassic bromide. In a third case of epilepsy, a cigar-maker, aged 25, who had occasionally as many as five fits in one day, had no return of the fits after taking daily fifteen grains of the sodium salt in three doses of five grains each; he also suffered from great depression of spirits and a "dull, heavy feeling."

In two cases of nervous excitement induced by mental anxiety the sodium bromide in small doses gave great relief. In a case of vertigo (probably epileptic) it did not seem to relieve the symptoms; whilst in the case of an old man afflicted with insomnia, the watery solution of the salt in ten-grain doses appeared to do positive harm. He was attacked with severe vertigo, fell down in the bed-room, and cut his head.

Notwithstanding these adverse cases, I think we have in sodic bromide a valuable nervous sedative, when it is judiciously used.

"WRITER'S CRAMP:" ITS PATHOLOGY AND TREATMENT.

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PART III.

IN considering cases of "Writer's Cramp" we have hitherto directed our attention to those instances (as being most common, in which certain muscles have been exhausted by excessive stimulation. It will be advisable, before considering the treatment of this disease, to point out that the irritability of a muscle may be lessened or annulled by causes other than that of over-stimulation, and that when the muscles thus affected are those of pen-prehension, we get a condition not to be distinguished from writer's cramp except by its history. I have lately had an opportunity of examining a gentleman who *suddenly* experienced great difficulty in writing. He is a writer in the Civil Service, and while engaged at a competitive examination he found one morning, on beginning his paper, that his handwriting was bad and shaky (much resembling the third sample of writing in the lithograph which accompanied the second part of this paper), that he was able to write only slowly, that he was obliged to grasp his pen very tightly, and that he was unable to adjust his pen between the fingers of his right hand without the help of the left. He experienced very little difficulty in other acts: there was no very evident loss of power in the hand, and on a cursory examination the case might have been mistaken for one of true writer's cramp. A careful examination at once revealed the true cause of his difficulty,

which was paralysis of the ulnar nerve, brought on apparently by prolonged compression by resting the elbow on the table while poring over a book. In this case, some of the normal muscles of pen-prehension were paralysed, and the big flexors of the fingers and thumb were supplying their place. All voluntary power over the interossei and other muscles supplied by the ulnar nerve was quite lost, and it was found that the reaction of these muscles to electricity was that which is usual in cases of peripheral paralysis; a too ready response to galvanism, but diminished irritability to faradism.

Another case, in some degree resembling the above, is that of a gentleman of independent means, who has been troubled with writer's cramp for thirteen years. He has never done much writing, certainly nothing which would be likely to overtire his muscles; he does not attribute his failure of writing-power to any cause, and cannot say precisely at what period it came on. On testing the power of the hand and arm, it was found that movements involving the use of any of the muscles supplied by the ulnar or musculo-spiral nerves were feebly executed and soon caused fatigue; and on further testing the electric irritability of the muscles of the hand and fore-arm, it was found that the muscles deriving their nervous supply from the median manifested a normal amount of irritability to both currents, but that those supplied by the musculo-spiral or ulnar responded too much to galvanism, and too little to faradism. The cause of this failure of power was not evident, but the appearance of some coppery spots upon the skin induced me to prescribe iodide of potassium; with what result I have not learnt.

A third case—one of true writer's cramp, but possessing a peculiarity which would remove it from the ordinary category—is that of a gentleman whose failure of power came on two years ago, while engaged in working very hard at writing out a course of lectures. He states that he has always had occasional difficulties in writing, and that even from a boy the act of writing soon fatigued him. One of the muscles most at fault was the long flexor of the thumb, which was not only weak, but visibly wasted. While writing he held his pen very tightly, with the distal phalanx of the thumb kept at a complete right angle with the shaft of the pen. On examining the hands, it

was found that both thumbs were almost absolutely stiff at the metacarpo-phalangeal joints, which did not admit of flexion or abduction. This immobility had, he said, always existed, and doubtless accounted for the necessity of the extreme flexion of the ungual phalanx during pen-prehension, and the consequent failure of the flexor longus pollicis.

I have been informed of two instances among musicians where prolonged strain, as in the case of M. Schumann, has resulted in loss of power. One case was that of a violin player who has lost the power of grasping, and consequently of fingering, the instrument with the left hand. The other case is that of a performer on the violoncello who has lost the power of "making the nut;" that is, of shortening the strings of the instrument by means of extreme pressure exerted by the phalangeal joint of the thumb, through the action of the abductor pollicis.

A few words may be devoted with profit to the mode of examining patients suffering from loss of writing-power. The lesion causing such loss of power is often extremely obscure; and the greatest care and the most painstaking and prolonged examination are generally necessary for its detection. My mode of proceeding with a patient is as follows:—First, it is necessary to ascertain certain facts as to the family history of the patient and the history of his ailment, and for this end the following questions should be put. Is he a scrivener, and does he habitually, or has he at any period of his life, exercised the functions of writing to an excessive amount? How long has the disease existed, and did it come on gradually or suddenly? Does the patient attribute the onset of the disease to any definite cause, and did it clearly follow any extraordinary effort of writing or similar exercise, such as etching, drawing, knitting? By the answers to these questions one is often able to get some insight into the case. If the patient be a scrivener, and if he attributes his loss of power to a definite cause, such as very excessive writing, one expects to find certain weak muscles. If the loss of power came on absolutely suddenly and after no extraordinary exertion, one would expect to find definite paralysis of one or more nerves. If the patient is not in the habit of writing excessively, and if the disease has crept upon him gradually and he cannot say definitely why or when, then one would

expect to find the lesion to be in the nerves rather than the muscles. A history of hereditary tendency towards writer's cramp or any other neurosis would of course materially influence both diagnosis and prognosis.

The next step is to strip the patient to the waist, and make a careful examination of the upper limbs, comparing one with the other. In this way any wasting of muscles will be detected, and the general state of muscular nutrition in both arms can be estimated. An examination of the nails, and the history which the patient gives as to the utility and quality of his nails, are of great importance as affording evidence of the state of nutrition. I have found thin, papery, brittle, pitted, or notched nails in at least half-a-dozen cases. In only one instance have I detected distinct evidence of wasting of any one muscle (the flexor longus pollicis), and even in this case it was a question how much of the wasting was due to a wristlet which the patient had been wearing. In the majority of cases, the arms have been flabby and the reverse of muscular, and in two cases the right arm was in a trifling degree more flabby and less muscular than the left.

Ask the patient to write, giving him a large sheet of paper, a good quill pen, and something to copy, and notice very carefully how he sets about it. Look to the mode of pen-prehension, and observe whether or no the pen is readily arranged in the right hand, and whether the assistance of the left is required. Any difficulty in this points to weakness of the interossei or other intrinsic muscles of the hand, for the patient finds an awkwardness in producing those lateral movements of the fingers which are necessary for the comfortable settling of the pen in the writing position. It is of course absolutely necessary to ascertain exactly how the pen is held, and what muscles are employed for that purpose. Patients who have the disease in an advanced form can only hold their pens by some method more or less grotesque, such as grasping it with the entire hand, or interlacing it in the way one interlaces a penknife when using it for erasing. In others the pen-prehension is more natural, perhaps quite natural, or possibly the first finger is not used, or the thumb has its ungual phalanx bent at a right angle. Notice whether the pen is tightly or lightly grasped. Place the fingers on the wrist, and feel if the flexor tendons are tense or lax.

Make him write with a soft cork penholder, and see if the force employed is sufficient to bend it. One of my patients snapped a cork penholder in two while trying to write with it. Is the pen too firmly pressed upon the paper? are the lines too thick? and can you see daylight between the nibs of the pen? Observations on these little points may each give its quota of information. Next as to stroke-making. How is it accomplished? Whether by the flexion and extension of the fingers and thumb as it should be, or by movements of the wrist or shoulder? Does the patient keep his wrist upon the paper, or is the fore-arm raised by the elevation of the shoulder? What is the position of the hand and fore-arm? Is it poised as it should be, or is the position that of complete pronation, as may be the case where the weakness of the supinators is very marked? Always look for associated movements; for although we may be unable to explain them, they are nevertheless, I believe, a very certain sign of muscular weakness somewhere. The tricks to which a patient has recourse to overcome his difficulties are generally most instructive. As an example I may mention the case of the gentleman (mentioned above) who had a semi-paralytic condition of the musculo-spiral nerve, who was only able to write by pressing his fore-arm close to the trunk because (apparently) of the weak uncertain action of his supinators and triceps extensor.

Lastly, it is of importance to notice when and why the patient fails in his writing. Some cannot begin to write, and fail almost immediately; others begin very well, and only fail after a certain time. In the first instance the irritability of certain muscles will be found more evidently below par than in the second instance. Some are stopped by genuine spasm, some by tremor, some by an extreme and agonising sense of fatigue extending all up the arm, or felt only at certain points. And lastly, some are stopped by the evident "giving out" of certain muscles. For example, in one of my patients with extreme weakness of the extensor *primi internodii pollicis*, the phalangeal angle of his thumb "gives way;" in another the first finger refuses to remain on the pen; while in others the hand rolls over towards pronation or supination, or the pen slips from the fingers and evades the grasp from no very evident cause.

Next proceed to examine the irritability of individual muscles by means of faradism and galvanism, and their power of work by the performance of voluntary muscular acts involving, if possible, only one muscle or group of muscles at a time. For testing the irritability to electricity of both kinds a small rheophore is necessary, and each of the small muscles of the hand must be tested separately and compared with its fellow on the opposite limb. Often a very decided lessening of faradic irritability is noticeable in certain of the normal or abnormal muscles of pen-prehension, but often the difference is not so much in the actual presence or absence of irritability as in the amount of contraction which follows the application of the faradic irritant. Occasionally it is found that some muscles not only respond too sluggishly to faradism, but that the same or others are too ready in their response to galvanism. The power of voluntary movement possessed by individual muscles should be ascertained by making the patient perform not only the coarse movements such as flexion, extension, pronation, and supination, but also a series of movements with each finger, so as to test the interossei; and by making him move the individual joints of his thumb, &c., always comparing the power of one side with that of the other. As a preliminary, the question should always be put whether or no the patient experiences any difficulty in performing muscular acts other than that of writing. A patient, in answering this question a few days ago, told me that he had considerable difficulty with certain acts, but not with others. "For example," he said, "I can't draw a cork out of a bottle because of the screwing movement." On questioning him still further, I found that he was not able, without giving himself pain, to unfasten a carriage door, and that for this act he invariably used his left hand. On examining him, it was found that the act of supination was performed with difficulty and occasioned great fatigue. In another case the exercising of the interossei muscles by separating and approximating the fingers was not only performed with difficulty, but its performance at once occasioned tremor. In performing these acts, it will be seen probably that some are far more readily performed than others, and that an intense feeling of fatigue is the immediate result of

the attempt to perform some exercises. Always look, too, for associated movements, which, as said above, are always a sign of weakness, and often only make their appearance when the weak muscle or group of muscles is called into action.

One other point remains to be determined, viz. the existence or not of a hypersensitive or neuralgic condition of any of the nerves of the arm; a condition which I have myself observed upon two occasions, and which Meyer mentions as occurring in three of the five cases which he records.

There is hardly any disease for the relief of which so numerous and such various therapeutic agents have been employed as for the one we have been considering, and we shall find that a consideration of the treatment may throw much confirmatory light upon the pathology. The object of much of the treatment which has been employed has been to contrive by some means to avoid using those muscles which have become irritably weak. For this purpose, Dieffenbach, Stromeyer, Aug. Tuppert, and others have practised tenotomy with the effect of relieving the patient for a time, but with the almost invariable result of the return of the disease after a greater or less interval. By tenotomy the patient is simply compelled to employ other muscles for pen-prehension, and it might possibly have been expected that the complete holiday which the tenotomised muscles enjoy would have the effect of restoring the irritability of the muscle by such time as, its tendon being re-united, it should again be called upon to exercise its functions. The results of tenotomy in this disease, however, go to prove that complete and forced inertness without any healthy exercise has not the power of restoring muscular tone.

Cazenave, and others who have imitated him, have thought to avoid the effects of the disease by providing the patient with some mechanical support for the pen, strapped to the hand or fore-arm. Means like this may enable a patient to continue to use his damaged limb, but it is evident that such arrangements are comparable merely to the starch bandage and the crutches which give to a man with a broken leg the means of progression. It is, however, impossible to write without a prolonged muscular strain somewhere; for even though the pen be strapped to the fore-arm, it is evident that the fore-arm

has to be kept in a fixed and steady position for writing, and it generally happens that the muscles used in steadying the pen, however big and near to the shoulder they may be, sooner or later give out, as did their diminutive predecessors in the hand.

Amongst the so-called cures for writer's cramp are the methods of holding the pen coarsely, as by grasping it in the hand like a field-marshal's baton, or by using an enormous penholder, such as a walking-stick. Again, various forms of penholders have been devised, and I know of one case in which the patient manages to write by means of a penholder shaped like a peg-top, which he grasps tightly in his palm (by means of the flexors of the fingers), but it is evident that by shifts like these the disease is no more cured than an obstruction at the glottis is cured by means of tracheotomy.

Almost the only curative agent which has acquired any reputation is *rest*, and all writers on this disease are agreed that rest is always of the greatest importance. By rest we mean abstention merely from writing, not forced abstention from muscular acts of all kinds, for in this latter way there is a risk of causing atrophy of the weakened muscles, and but small chance of improving their tone. If the disease is treated early in its course, and the patient is able upon its first appearance to abandon writing for a time, the muscles recover their tone, and he is able to resume his occupation after an interval of a month or two. Patients in whom symptoms of writer's cramp have ever appeared should always be warned that they will never be able to perform an excessive amount of writing, and that any attempt to do so may be followed by disastrous consequences. If his occupation be that of a writing clerk, it is, I think, absolutely necessary for his future happiness that his employment should be changed, perhaps not entirely, but lightened at least, by finding some employment which may alternate with the periods of writing. To allow a patient with writer's cramp to continue his excessive writing is like allowing a dyspeptic to continue with the diet which we know is the cause of his dyspepsia. I may here perhaps remark that I believe there are very many cases of incipient writer's cramp to

be found in public and private offices, and it is very common to find that a clerk begins to hold his pen in some queer way merely because he gets on better with the new method than by the old. If such cases were carefully examined, I believe we should often detect a weakened muscle. It is a matter of fact that writer's cramp rarely comes under the eye of a physician till it has existed months or years and has made considerable advances. The patients go on for indefinite periods of time, merely thinking that their hindrance arises from their own stupidity, and it is only when the various shifts for relieving one muscle after another have been unconsciously employed and exhausted, that the patient seeks advice.

Liniments and douches have been often prescribed, and in so far as they may quicken nutritive processes, I believe them to be useful. One of my patients with a neuralgic condition of the musculo-spiral nerve experienced some relief from using an aconite liniment.

Gymnastic exercises have been used and have enjoyed a certain reputation, and I believe that if such gymnastics be directed towards the rhythmical exercise of the affected muscles, much good may be done. The muscles become weakened and irritable because, in the words of Sir James Paget, "the rhythm of nutrition is perverted;" and I believe that by the employment of rhythmical exercises very much good may be done.

With regard to drugs and constitutional treatment, I will say that everything which improves the patient's general health is capable to some extent of improving his local ailment; and while his local ailment is under treatment, it is absolutely necessary to regulate with great care the functions of the body. The nervine tonics are all useful, but must not be used indiscriminately. I have found strychnine of great service in one or two cases where the patient's general health was much below par, and was accompanied by a state of mental depression. In highly nervous excitable people, and especially in one case where there was distinct muscular spasm,¹ it was found that strychnine served merely to increase the patient's condition. In such cases, and especially when the patient is excitable and anxious, nervous in the presence of others, and a bad sleeper,

¹ See *Practitioner* for September 1872.

I have found the greatest possible service from the administration of bromide of potassium.

With regard to local treatment, the same principles should guide us as in the treatment of all other diseases, viz. remove the cause, and heal the damaged parts. The patient with advanced writer's cramp should be forbidden to write at all for a time; but I see no objection to his doing a moderate amount of writing with the left hand while the right is under treatment. The mechanism of writing and the cause of failure should be fully explained to the patient, a proper and normal prehension of the pen should be insisted upon, and he should be prevented from grasping the pen too tightly and from using for this purpose any but the proper muscles. This may be done partly by precept and partly by making the patient write with a soft-pointed instrument. The use of quill pens should be insisted upon, or a soft pencil or fine-pointed paint-brush may be used for practising writing. The patient is generally in the condition of a child as regards the act of writing. Not only has he to learn afresh how to hold and move his pen, but he has also to unlearn the vicious modes of writing into which he has been forced by circumstances.

With regard to the local treatment of the affected muscles, we have, I believe, two means of materially improving their condition. One is the application of electricity, and the other is rhythmical exercise. With regard to electricity, I believe that galvanism continuously applied is preferable to faradism. I am well aware that faradism has been used with advantage by Zuradelli, Meyer, Haupt, and Dr. Buzzard; but I feel that faradism must be rather a dangerous application, and that by using so powerful a stimulant we run no small risk of exhausting what little irritability remains in the muscles. In the hands of most physicians faradism has done no good, and Duchenne says very definitely that its employment, even by himself, has not been followed by success. It will be remembered, too, that of Dr. Buzzard's two cases reported in the *Practitioner* for August last, one appeared to be a case of definite local paralysis rather than true writer's cramp. The great use of faradism is undoubtedly in cases where voluntary movement is no longer possible, but where voluntary power remains we are by no means sure that

artificial stimulation of the muscles is to be recommended. I believe, and have, I think, to some extent proved, that the continuous application of galvanism is of the greatest service in writer's cramp. There can be little doubt that the galvanic current has a powerful influence on nutrition. After passing a current through a limb, by putting the positive sponge over the brachial plexus in the axilla, and allowing the hand to rest in a basin of tepid water connected with the negative pole, the limb feels warm and comfortable, and any painful feeling of fatigue is removed. Patients often express the greatest pleasure during this manœuvre, and this fact alone is some indication of its therapeutic value. Another advantage which the galvanic has over the faradic current is that it allows of voluntary muscular action during its continuance, and rhythmical exercises of the affected muscles can be practised at the same time that the current is being passed along the nerve supplying them. It certainly seems to me more reasonable and scientific to make use of the natural stimulus as long as we can, rather than have recourse to a stimulus which is wholly artificial. Again, I am sure that the power of voluntary movement in any muscle or group of muscles is very much increased by the passage of a mild galvanic current through the nerve supplying them. The current must be mild, so as to produce a maximum amount of cathelectrotonus in the part of the nerve included by the poles of the battery. It has been shown as a laboratory experiment that a nerve in this condition has its irritability and conductivity heightened, and the experiments which I published in the January number of the *Practitioner*, as well as subsequent clinical observation of the fact, have convinced me of the truth and great practical utility of this point; and it will be found that certain movements which, when the patient is unaided, are accomplished only with difficulty, are effected with comparative ease when the current is transmitted along the nerve. My mode of using the current is as follows:—One pole (the positive) is placed, let us say, in the axilla, and the other over the ulnar nerve just where it leaves the edge of the biceps muscle *en route* for the olecranon. The strength of current is short of that which causes muscular contraction, but is just sufficient to make the patient conscious of a tingle in the end of the little

finger when the circuit is made or broken. The patient is then made to exercise the interossei by separating and approximating the fingers rhythmically. Take another example:—The positive pole may be placed over the median nerve at the inner border of the biceps, and the negative over the belly of the flexor longus pollicis, while the patient is made to flex rhythmically the distal phalanx of his thumb: or again the positive pole may be placed high in the axilla, and the negative over the musculospiral nerve as it turns forward alongside the supinator longus just above the bend of the elbow; and the patient is then made to supinate the hand or extend the fingers rhythmically.

Again, the anodyne effects of the galvanic current are well known, but there is probably no form of pain over which it has such complete control as the pain of muscular fatigue, the form of pain, be it observed, with which we have to deal in writer's cramp. Its power over spasm, one of the occasional complications of writer's cramp, is also well known.

The effect of this mode of treatment in two of the cases which I have treated myself has been most marked, the patient's alleviation following immediately on the first application. In two other cases which were under my care about a fortnight, and in which considerable writing-power existed, the chief symptoms being tremor, total alleviation resulted in one case, and considerable diminution of the tremor in the other. With regard to the other cases I can hardly speak, as I only saw them in the course of consultation. With regard to treatment, one must remember Trousseau's dogma, *Longue maladie, longue traitement*; and it is not rational to suppose that a condition which has often existed for years before it comes under the eye of the physician can be cured without many months of patient and systematic treatment. The worst two cases I have seen, the only two, in fact, in which the loss of writing-power was total, have been treated by myself in the manner indicated, and with marked success. One of these will be found recorded in the number of this journal for September 1872. I propose concluding this paper with a few notes of the other.

Henry Millerd, aged 40, in the employ of the General Post Office, came to consult me at the Charing Cross Hospital on October 5th, 1872. He is a "nervous" man, has suffered from

neuralgia, and has one sister who suffers from paraplegia. Has done an immense amount of writing during the last thirteen years, often writing "against time." States that in the summer of 1868 he felt that he had lost control over his pen, and was obliged to grasp it tighter. He managed, however, to continue writing tolerably well till April 1872, when "he lost control over the fore-finger," and was obliged to begin holding his pen by interlacing it among the fingers. He cannot rest his wrist upon the desk when writing: if he does so, the fingers and thumb "start away from the pen." He complains of deep-seated pain in the wrist-joint, and in the carpo-metacarpal joint of the thumb. He has now completely lost the power of writing with his right hand, and has learnt to use the left; but, strange to say, he is quite unable to write with the left hand without making spider-like movements with the fingers of the right.

Any attempt to use the right hand causes agonising pain from the shoulder downwards. The pain is the pain of intense over-fatigue, and does not follow the line of the nerves. On stripping him it was found that there was nowhere any detectable wasting of any muscle, but that both arms were soft and flabby, and the right rather smaller than the left. On being asked to write, the whole body seemed to take part in the exertion; the pen was grasped as tightly as possible, the fore-arm raised off the table, the shoulder elevated. An incomplete signature was all that could be effected, and the attempt caused great pain in the limb. On testing with faradism, it was found that the inter-ossei, the abductor pollicis, the flexor longus pollicis, and the extensor primi internodii, contracted far less readily and less forcibly than their fellows of the opposite limb. Voluntary movements of these muscles were performed with the greatest difficulty, as was also the movement of supination, which could only be effected four or five times in succession, notwithstanding the strongest efforts to do so. The phalangeal joint of the right thumb has a curious habit of "snapping" audibly at intervals, which is due, apparently, to the spasmodic and unequal action of the muscles moving it. The nails of both hands are very bad being "pitted" in an extraordinary manner.

His condition is ever present to his mind, and his mental state is one of great depression, his writing-power being his

source of income. Lithographed fac-similes of this patient's handwriting are given, which show how rapid was his improvement under treatment. On the first application of the current the pain in his arm disappeared, and within a week of its first application he could manage to sign his name with tolerable ease. His writing from this time rapidly improved, and at present his handwriting, when he is allowed to use his right hand (which is very seldom), is, as far as appearances go, as good as it ever was. Samples of his handwriting are given in the accompanying lithograph. The first three samples were written respectively on the 5th, 12th, and 21st of October, and No. 4 on the 1st of November. The other two samples are dated. Although the handwriting is now very good, the pen-prehension is still faulty, and I have forbidden him to write until he can do so with the pen held properly and lightly between the thumb and first two fingers. Pen-prehension has gradually very much improved, and it was soon noticeable that the effort of writing had much decreased, and such efforts soon ceased to cause fatigue. First, the elevation of the shoulder disappeared; then the fore-arm could be rested on the table during writing. Next, the phalangeal angle of the thumb no longer gave way, and now he can write very comfortably if he holds the pen only between the thumb and first finger (at one time the first finger could not be placed on the penholder), but any attempt to use the middle finger bothers him very much. On testing the muscles with faradism, it is now found that the dorsal interossei on either side of the middle finger respond *far less* readily than their fellows of the left hand, but that the other interossei, which were weak in the first instance, have recovered their normal irritability. Earlier in the case it was evident that the dorsal interosseous muscle on the radial side of the middle metacarpal bone was less irritable and more weak than the one on the ulnar side; and by placing a rheophore on the metacarpal bone so as to influence equally the muscles on either side, the first phalanx was flexed, but dragged by the stronger muscle towards the ulnar side.

The galvanism has been used in the manner stated above, and the variety of exercises which he has been made to practise is very great. Especially he has been made to hold a pen or pencil,

or my finger, and to perform with it the movements of writing a hundred times in succession, while the current has been applied alternately to the nerves supplying the muscles implicated. At first the difficulty of this exercise was extreme, the pen frequently slipping from the grasp of the fingers. At present it can be performed without any difficulty whatever. The only movement at present in performing which there is the slightest difficulty is the waggling of the middle finger from side to side by means of the interossei. It was noticeable very early in the case, that the fore-arm had got firm and muscular, and that the pitting had disappeared almost entirely from the finger-nails. The patient's enjoyment of the current is quite laughable, and during its application he frequently uses such expressions as "*That's comforting*," "*That seems to give me strength*," &c. The general health has much improved, a fact which is attributable in great measure to the removal of the cause of his mental depression, as well as to the mixture of strychnine, perchloride of iron, and nitro-muriatic acid which he has been constantly taking. The snapping of the thumb has almost disappeared, and is now far less frequent and less loud.

He has continued to use the left hand for writing while he has been under treatment. The movements of the right fingers while writing with the left hand have quite disappeared. Once or twice he has complained of a feeling of awkwardness with the left hand, but this has always been immediately removed by the current. The amount of writing which he has done during the treatment has been about two hours a day, but this amount had to be performed often rapidly and *against time*.

In conclusion, I may be allowed to express my belief that chronic fatigue from over-strain will be found to be the cause of certain other progressive functional disorders. In fact, I have at present under my care a case of clonic torticollis, which I believe to have been clearly caused by the over-straining of the left sterno-mastoid and trapezius. I shall hope at some future time to touch upon this subject and report the above-mentioned case.

MIGRAINE FROM INJURY TO THE HEAD.

BY DOUGLAS MORTON, M.D.,

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I WILL offer a number of facts, drawn from my own personal experience of migraine, that throw, I think, a flood of light upon part of the subject out of which arose the discussion between Dr. Anstie and Dr. Dale.

In my judgment these facts very distinctly support the theory of a neurotic origin. The symptoms attending all my attacks, in order and character, have been just the same ; and each attack, as far as I have been able to learn, may be properly regarded as a *typical* expression of the affection. But the point to which I would especially call your attention is, that *my first attack was directly and immediately induced by an injury.*

About fourteen years ago, while a student at college, I had a severe fall from a swing in a gymnasium, my head striking with great violence the hard, beaten earth beneath. At the moment I felt no pain, but experienced a horrible sensation, which I suppose is best described by the word *stunned*. I went at once to my room, and when, after a short time, my symptoms had improved a little, I took up a Greek book in which I had to prepare a lesson for that day. To my dismay I found that I could see only the left half of the open book ; and if I directed my attention exclusively to a line or a word, only the left half of that. My first impression was that my right eye had been injured ; and when I looked in a mirror to find out if this were the case, I was startled at not being able to see any part of the right side of my face. I held my hand over one and then the other of my eyes, and discovered, as I supposed at the moment,

that the injury to my eyes consisted in destruction of vision in the right halves of both, for with either closed I could see the left halves of objects I looked at with the other. As yet, as well as I recollect, my headache had not commenced. I returned to my book and tried to translate, but found this impossible—the meanings of the words had escaped my memory almost entirely. I could scarcely pronounce a word—the difficulty seeming to be that I could not well retain a conception of the sound represented by more than one letter at once. In a short time I began to suffer headache, but at first it was not severe, and I went out and joined a group of students on the college grounds, in whose conversation I ventured to participate. I found, however, that I was the subject of a peculiar kind of aphasia—I could intelligently follow their conversation, but had not the power to select words suited to the expression of what I wished to say. I hit upon words in a most curiously random way—a verb, perhaps, for a noun, or an adverb for a preposition. My headache continued to increase in severity, and in another hour became almost intolerable. Now I began to suffer nausea, and vomited, but the disturbance of vision had entirely passed away. I soon recovered from the sickness, though the headache lasted the remainder of the day—about eight hours.

Had I suffered no subsequent attack of migraine, I should now regard the condition which I have described as simply that to which “concussion of the brain” gives rise—the subjective symptoms of which, by the peculiar circumstances of my case, I could observe closely, and give a better account of than we can usually elicit from patients.

After an interval of about seven years I had my second attack, and on this occasion noticed phenomena that escaped my attention in the preceding one, viz. the appearance of such a figure as I have represented in the enclosed drawing,¹ trembling in space, seemingly a few feet from my eyes, and a sensation of numbness in my right cheek and the right side of my tongue, which came on with the headache (if my memory is correct) after the optical phenomena had disappeared.

Since that time I have had several other attacks, each being expressed by essentially the same set of symptoms. The form

¹ No drawing found in envelope.—ED. *Pract.*

even of the phantasm which quivers before my eyes in the outset of every attack has continued almost exactly the same.

On studying the etiology of my case, I have not met with the success Dr. Dale appears to have met with in his. In no instance have I been able to connect an attack with any deviation from what is my usual mode of life, whether as to diet, amount of work, mental or physical, amount of sleep, or exposure to weather. Nothing whatever in my experience has seemed to point to disturbance of digestion as a cause. I have suffered headache from the latter cause, and I suppose from most of the causes that usually occasion headaches in other people; but my *sick-headache* stands alone—*sui generis*—in its peculiar group of symptoms and in the obscurity of its etiology.

In my experiments in treatment I have been more fortunate; indeed my results have been positive, and, happily, decidedly favourable. I have experienced benefit from taking a cup of strong tea at the occurrence of the first symptoms; so, also, from a tumbler of sherry wine; but *the* remedy *par excellence* I have found in guarana. The specimen of this drug that I got was put up in a package, containing twelve powders, of, I suppose, about thirty grains each. I took four of those in two hours, and, though perfect relief was not afforded, my suffering was yet diminished to such a degree, that the attack, whose prodromes foreboded unusual severity, proved to be a very small affair indeed. Having tried the guarana only once, it may appear unscientific to speak so enthusiastically of it as *the* remedy; but so intense had always been my suffering, and so great was the relief following the medicine, that it is impossible for me to regard their connection as otherwise than a *propter hoc* relation. I have seen one or two other cases reported in which great benefit was derived from guarana. The remarks I have made upon treatment, here, however, are of secondary consideration, my main object being to give the circumstances attending my first attack, as, to my mind, they appear to throw light upon the true character of the affection.

SLIGHT POISONING FROM THE MEDICINAL USE OF PHOSPHORUS.

BY DR. ANSTIE.

At the present time, when attention is being widely directed to the efficacy of phosphorus in nervous diseases, it may be useful to record every incident that throws light on the as yet undecided question of dosage. In the interesting paper by Mr. Ashburton Thompson, in the July *Practitioner*, it was shown that at any rate a great many patients will bear doses of phosphorus from two to four times as large as those which have been in use during the last few years. I, on the other hand, have to record a case which shows that we may meet with an occasional patient in whom there is a greater sensitiveness than usual to the poisonous influences of the drug.

A gentleman, aged 33, single, applied to me first in May 1872. He had been subject to migraine once a month (or oftener, under circumstances of special fatigue) in boyhood; but the pain was more or less diffused, and not very evidently neuralgic in type, till the age of nineteen. Since the latter date it has been strictly confined to the left side of the head (anterior two-thirds); occasionally it extended into the globe of the eye, and sometimes slightly invaded some descending cervical twigs. It never was felt in either maxilla.

The family history was decidedly bad. A brother was neuralgic; the mother died mentally deranged; a sister is partly insane.

Up to December 1871 the attacks of pain were always attended with vomiting; in that month there was a very bad

attack of typical sick-headache, and then the vomiting altogether ceased to occur. Before the change just mentioned, the attacks were always preceded by vertigo; since that date the vertigo, like the sickness, ceased to appear. There never has been any marked lachrymation; but it is noteworthy that since the attacks have become more like ordinary neuralgia than migraine, the tendency to conjunctival congestion has markedly increased. On questioning the patient as to the nature of the vomiting that formerly occurred, he was quite clear in the statement that no solid food was ever thrown up; the sickness always took place in circumstances of fasting, and the vomited matters were watery and sour. He has not been blessed with a very good appetite; but, on the other hand, there is no history of anything that can be called dyspepsia.

As regards the remedies which had so far been employed, he had taken (among other things) quinine, iron with strychnia, and also arsenic; the latter did him more good for a time than anything else. He had for some years been in the hands of a very intelligent practitioner, and had not suffered from the injudicious kind of treatment to which, unfortunately, migrainous patients are too often subjected.

In advising on this case, it became important to consider the circumstances of the patient's life. He was engaged in a business which often occupied him for many hours in a chiefly sedentary manner, and had likewise evidently undergone much anxiety and worry. I suspended, for the time, the question of his taking an entire leave of absence from business for a prolonged period, but insisted on his taking things more easily. He was recommended to take a steady course of arsenic and cod-liver oil, and to increase his allowance of food.

Early in May of this year the patient's ordinary medical attendant called me to him in consultation. It appeared that he had been better, on the whole, during the past twelve months; still, he had not shaken off the neuralgic tendency, and it became a question whether any further medication should be tried, or whether he should at once be sent on a prolonged sea-voyage. I was anxious to give phosphorus a trial; and on May 5th, accordingly, our patient commenced taking, thrice daily, a pill containing $\frac{1}{16}$ th grain. On July 8th his medical

attendant brought him to my house, with the following interesting history:—

From May 5th to May 11th or 12th he took the pills regularly, but during the two last days suffered from a constant and increasing burning sensation at the epigastrium. What led to the suspension of the phosphorus was the fact, to which the patient himself drew his medical man's attention, that the urine was dark-coloured: on examination it proved to be both albuminous and bloody, but very careful microscopic examination failed to detect any casts. After a day or two's interval the renal symptoms had almost disappeared, and the stomach was much easier, though the patient felt (and, indeed, still feels slightly) a rather burning pain between the scapulæ. As to the neuralgia, this is remarkable:—On the whole he had suffered more during the phosphorus-medication than previously, but it was evident that some powerful influence had been exerted on the nervous system, for the rhythm of recurrence of pain was quite changed. There were now two exacerbations daily, but the pain was altogether more of a continuous and less neuralgic type. On the 14th, after two days' rest from the phosphorus, he took one five-grain dose of quinine about an hour before the morning exacerbation was expected, and, singularly enough, never had a recurrence of actual pain afterwards, though there were a few occasional threatenings. He went to St. Leonard's for a little while, and this seemed to help him; and on the 8th July, when brought to me, he was quite free from his neuralgia; but, though fatter than he had been, still undeniably thin. A stranger to the history of the case might suppose that the single dose of quinine had been the cause of the cure; but this seems impossible, for he had been saturated with quinine by his own medical attendant (before I saw him first) without the slightest benefit. At any rate one may say that the quinine would probably not have done him any good, had not a strong impression been previously made by the phosphorus.

I think that it is very evident that poisoning by phosphorus did occur in this case. The burning pain in the stomach, together with temporary hæmaturia, at once relieved by discontinuing the medicine, seem to leave no doubt of this; and thus we must acknowledge it possible that a patient may suffer the

slighter degrees of phosphorus-poisoning from less than three-quarters of a grain in minute doses distributed over seven days. At the same time, in view of Mr. Ashburton Thompson's use of the drug in much larger doses, for a large number of patients, without any bad result, we must suppose that the occurrence is quite exceptional, and due to an unusual susceptibility.

THE EAUX-BONNES AND PULMONARY PHTHISIS.

BY M. LE DR. LEUDET,

Physician to Eaux-Bonnes, &c. &c.

MODERN histological researches on the texture and organisation of tubercle, its origin, development, and terminations, have not only done service to science in affording us a better acquaintance with the morbid product which anatomically characterises pulmonary phthisis: they have also had another result, far more valuable for the medical art, that of strengthening in the medical man his belief and trust in the curability of a chronic disease, the most universally prevalent on the surface of the globe.

Without troubling our minds to decide whether all the lesions which we meet with in the lungs of a phthisical subject are of the same nature, or, on the contrary, are of different kinds—one sort properly called *tubercular*, the other simply *phlegmatic* or *inflammatory*—it is quite certain that these lesions can no longer form a morbid entity subject to fixed and invariable laws. The microscope, enabling us to assist at the first appearance of tubercle, by permitting us to follow out its evolution from its birth to its death, has returned to irritation and inflammation the office which Laennec and his disciples denied them. Laennec's tubercle, immutable, fatal, *specific*, can no longer be admitted now-a-days. Far from being original and specific, tubercle is but a common and vulgar production, miserable and degraded. It resembles pus: like pus, it is engendered by numerous and varied causes; like pus, it is attended by inflammation and fever. It may be said of tubercle that it is a profound pus—destructive, organic, and constitu-

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muscular coating of the bronchi, excite its contractions, and may go so far as to create artificial asthma, which becomes a powerful antagonist of the tubercular lesion, invests this lesion, and prevents it from extending.

There are two symptoms which play a most important part in the history of pulmonary phthisis: *hæmoptysis* and *fever*. Is then the thermal medication of the Eaux-Bonnes, so profoundly exciting, really applicable in cases of a disease like this, which is almost always febrile, and so often accompanied by congestion and hæmorrhage in the pulmonary parenchyma? It is quite obvious that, in these cases, the question is to be able to choose those forms of the diseases which can be treated by the sulphurous medication.

It must be understood that I do not in any case refer to the acute forms of the disease which all formally counter-indicate the use of the Eaux-Bonnes: I refer exclusively to the chronic forms. Well: if the fever is more prominent than the pulmonary lesion; if with an insignificant tubercular lesion there exists intense, continued fever, as ardent in the morning as, or even more ardent than in the evening, the Eaux-Bonnes must not be employed—they would accelerate the disease instead of arresting it. For, indeed, these are cases where the entire organism is more tubercular than the lung itself, and it is exactly the opposite cases which suit the Eaux-Bonnes treatment. But, on the other hand, when we meet a phthisical subject with small caverns, or even a vast one, in his lungs, and with only moderate fever, he can safely drink sulphurous waters and derive a veritable amendment from the thermal treatment. It is not, therefore, as a *symptom taken in itself* that fever counter-indicates the use of the Eaux-Bonnes, but on account of the morbid state which it expresses. Thus with grave and extensive tubercular lesion of the lungs, and fever in proportion to the lesion, the Eaux-Bonnes may be prescribed. On the contrary, with intense and unremitting fever, and little advanced pulmonary lesion, they must be severely proscribed.

Hæmoptysis, *per se*, is not, any more than fever, a counter-indication to the use of the Eaux-Bonnes, as it is, after all, but a symptom of phthisis, like cough, loss of flesh, &c. There is no doubt that the Eaux-Bonnes may bring on spitting of blood

in phthisical subjects, since they are capable of producing the same result in individuals whose air-passages are quite unimpaired. In describing just now the particular excitement caused by the Eaux-Bonnes in the bronchi and lungs, I said that expectoration was modified almost at a given moment of the thermal treatment. I will add that often, when the case is one of phthisis, the sputa become pinkish, occasionally bloody, and sometimes consist of pure blood. Whether the medication be suspended or not, this slight hæmoptysis lasts only twenty-four or forty-eight hours, and does not appear at any time again during the course of treatment. It is therefore, in reality, only a pathogenetic effect of the sulphurous water, without any ill influence on the course of the disease. I even think that these spittings of blood, produced by the thermal treatment, are nothing but a substitutive irritation, exerted on the bronchial mucous membrane and often followed by good results. I have seen certain cases of hæmoptysis, occurring during the course of treatment, followed by extraordinary amendment in the general and local condition of the phthisical sufferer. One of these cases, a remarkable instance, was published by me in the *Gazette des Hôpitaux*: the patient was under my treatment or observation for four years; he is alive, has paid no attention to his health for nearly the last ten years, and practises the hard and fatiguing profession of a country doctor.

One of the great advantages of the thermal medication of the Eaux-Bonnes in the treatment of phthisis is certainly its singular property of reviving chronic diseases and bringing back various morbid manifestations which had disappeared since the super-vention of pulmonary tuberculosis. And here I will make a few remarks touching certain varieties and forms of phthisis which we meet with daily at the Eaux-Bonnes.

It is quite obvious that when, after one or several seasons passed at the Eaux-Bonnes, we notice in a phthisical subject, whose local condition has been modified, either an eruption on the skin, or the appearance of migraine or piles, or certain cases of arthropathy, or again biliary or nephritic calculi, &c., such morbid manifestations have not been created wholesale by the mineral water, and the use of the water could never have brought them into existence if these morbid manifestations

were not the returning vestiges of a constitutional disease, used up, altered, and vanquished by a more destructive and fatal diathesis—namely, tubercular diathesis. The wakening up and return of some of the elements of this chronic disease, less fatal than tuberculosis, are certain symptoms of recovery of the organism. They counterbalance the more fatal destructive disease, which may thus be permanently checked in its fatal course.

It is quite easy to indicate the forms and varieties of pulmonary phthisis which are especially amenable to the thermal treatment of the Eaux-Bonnes, since we are acquainted with the remarkable pathogenetic effects of this medication on the skin, liver, kidneys, &c. These forms and varieties may all be included under one heading—*Cases of Incomplete Phthisis*. By this name I mean those cases of phthisis in which tubercle does not constitute the entire disease, where the lungs and the organism are not equally abandoned to phthisic destruction, in which, notwithstanding extensive lesions of the pulmonary tissues, the economy is yet standing and resistant. Such cases are numerous, and nearly everybody has witnessed them. But what reason is there for this inequality between the pulmonary lesions and the general disorder? Why is the organism yet comparatively healthy, when two-thirds of the lung are already destroyed? It is because a foreign element, and a less fatal one, is associated with tubercle; because the patient is not only phthisical, but something else besides—rheumatic, gouty, or herpetic.

This association of two or several constitutional diseases in the same individual—tuberculosis on the one side, and gout, for instance, on the other—constitutes precisely what I call *incomplete phthisis*. The relation of phthisis with other chronic diseases, and their influence on the character and course of pulmonary phthisis, have been observed, studied, and differently appreciated by medical authors who have written on tubercular diathesis. A case of mine causes me to adopt the opinion of those who, like Dr. Pollock in his "Elements of Prognosis in Consumption," Dr. Noel Guéneau de Mussy in his "Memoir on Asthma and Empyema," and lastly my friend and master, Dr. Pidoux, in all his publications on Phthisis more than thirty years since, who

believe that the association of phthisis with other less grave and less disorganising diseases acts powerfully in modifying and arresting the tubercular evolution.

All phthisical subjects, therefore, who present vestiges of arthritism or herpetism, who are gastralgic, *migraineux*, hæmorrhoidal, &c. ; all those who, without being rheumatic or asthmatic themselves, are of gouty or herpetic families, will find in the use of the Eaux-Bonnes a useful and favourable medication. The Eaux-Bonnes will develop in them affections which counter-balance tuberculisation ; they will increase their elements of resistance, in exciting and bringing back to life morbid manifestations which are antagonistic to tuberculosis.

I insist on these cases of incomplete phthisis, as, during the fourteen years that I have observed them, from the point of view of the antagonism which other constitutional and chronic diseases exert against them, I have been convinced that they bring out a pathological law of extreme importance and most fertile in consequences as to the prognostic and therapeutical indications of pulmonary phthisis. I do not pretend that all these cases of incomplete phthisis will equally benefit by a thermal course of Eaux-Bonnes. Neither do I mean to say that there is no other treatment to apply to them except the Eaux-Bonnes. I believe, on the contrary, that the therapeutics of pulmonary phthisis must be as varied and complex as the causes which bring on the disease—causes which must never be lost sight of, if we wish to attack the disease surely. But I say that the thermal medication of the Eaux-Bonnes, by exciting the phenomena which retard tubercular evolution, by awakening affections which prevent the too rapid progress of tuberculisation, constitutes one of the most powerful therapeutical means to oppose to pulmonary phthisis.

The Eaux-Bonnes constitute therefore an exciting medication, and their action is at once local and general.

1. Locally, they act by modifying profoundly all the tissues of the respiratory apparatus, by tonifying the bronchi, and by exciting in the mucous membrane healthy or medicinal irritation, which supersedes the morbid irritation of tubercular catarrh. This medicinal action of the Eaux-Bonnes on the pulmonary

tissue can in no way surprise us, as the bronchial mucous membrane is the great eliminating surface for sulphur.

2. Their general action is more complex, but not less powerful. Whilst all the nutritive and eliminating functions, as well as the secretions, are excited and acquire fresh energy, old *organic habitudes*—hæmorrhoids, neuralgia, cutaneous herpes, &c.—are awakened, and oppose a check to tuberculisation.

The phthisical sufferer who goes to the Eaux-Bonnes does not only meet with a special medication, he also finds there hygienic, topographic, and climatic conditions which are eminently useful and salutary, and form the complement of his thermal cure.

The station of the Eaux-Bonnes, 800 metres above the level of the sea, is situate at the bottom of the Valley of Ossau, and completely sheltered against all winds by the mountains which surround it on all sides. The climate is sedative, temperate, and humid, suitable for weak and delicate chests. Our phthisical patients live there mostly out of doors, go up to the source in all weather, and express surprise that they do not catch cold when we compel them to walk out, notwithstanding the rain and bad weather. But with this rain the atmosphere is always calm; there is scarcely ever any wind—so little, indeed, that several days may pass without noticing the leaves of the trees to move. This calm is obviously good for phthisical subjects, and contributes to strengthen them and ameliorate their condition.

As to the hygiene carried out by our patients, it consists in life in the open air, daily walks, riding exercise, and nourishing and restoring food.

After the water-cure we send our patients to one or other of the two ocean stations near the Eaux-Bonnes, namely, Arcachon and Biarritz, the climate of which, during the months of October and November, is so tonic and mild. Or, again, we send them to Switzerland, to get the benefit of a grape-cure or a whey-cure.

Reviews.

Elephantiasis Græcorum, or True Leprosy. By ROBERT LIVEING, A.M. and M.D. Cantab., F.R.C.P., Physician to the Middlesex Hospital. The Gulstonian Lectures for 1873, revised and enlarged. London: Longmans, 1873.

THIS is a careful and painstaking study of the history, present geographical distribution, etiology, clinical history, morbid anatomy, and treatment of leprosy. Dr. Liveing has evidently taken much pains to collect information, from numerous sources, which might throw light upon this most interesting and mysterious malady; and he gives very careful records of some cases which have been under his own care in the Middlesex Hospital. As regards the important question of etiology, the following is the summary of his conclusions:—(1) The primary cause is yet unknown; (2) of subordinate causes, diet and hereditary tendency are by far the most important; and climate, soil, and race are not without a certain influence on the development and progress of the disease; (3) it is uncommon before puberty, and is, in some countries, more prevalent in the male than in the female population; (4) leprosy, if not contagious, is capable of propagation by the imbibition of the excreta of lepers. Upon the subject of treatment Dr. Liveing does not seem to entertain any very hopeful views; preferring to rely upon prophylactic measures. We are rather disappointed that he does not dwell more upon the influence of malaria, both as a cause of leprosy and as giving valuable suggestions for treatment; as also that he does not refer to the interesting researches of Dr. Reid and others which brought into prominence the importance of the localised muscular atrophies which occur, especially in connection with anæsthetic leprosy. Probably Dr. Liveing, who is far too careful a student of the literature of disease not to be well aware of the new facts which appear to bring these subjects into important relief, does not consider the evidence sufficiently copious or weighty to demand much discussion. We confess that we are of a different opinion, and think that they will hereafter command a great deal of attention. But, in other respects, Dr. Liveing's treatise is to be recommended as a careful and impartial study of the disease,

related in clear and elegant English. It was a very appropriate thing that the Gulstonian Lecturer should have chosen a subject on which the College of Physicians has expended (through its committee) such special and fruitful labours; and Dr. Liveing's treatise will be received as a welcome contribution to the mass of information which is now accumulating with regard to leprosy.

Clinic of the Month.

The value of Carlsbad Waters in Affections of the Liver.

—Dr. Stephen Ward, after describing the favourable situation and surroundings of Carlsbad, states that the Sprüdel spring has a temperature of about 165° F., and from twelve to forty-eight ounces of the water are drunk by the patients before breakfast, or between 5 and 8 A.M. The Sprüdel water, he goes on to say, contains, in sixteen ounces, from eighteen to twenty grains of sulphate of soda, about ten grains of carbonate of soda, and a little carbonate of lime and carbonate of iron. It is also charged with a certain amount of carbonic acid gas. In regard to the therapeutical action of a water the principal ingredients of which are sulphate and carbonate of soda, it would appear that in consequence of the power of the former to retain the water contained in the intestines, it loosens and carries down dried fæcal masses, tenacious mucus, bile, &c. The alkaline carbonate would act by neutralising free acids in the alimentary canal, by exciting the secretions of the intestinal glands, and it would also assist in determining the diuretic action of the water. The Carlsbad waters do not act so rapidly or produce such fluid stools as the Püllna and other waters in which sulphate of magnesia forms a prominent ingredient. They, however, even when first taken, induce an effective emptying of the bowels, causing pulpy, slimy stools of dark colour and offensive odour. The action is as a rule perfectly easy. If taken in excess, teasing watery stools may result. Purging is not essential to their favourable action. (*Lancet*, July 5, 1873.)

Nitrate of Potash in Acute Pneumonia.—Dr. Macnaughten Jones collects and publishes some notes of cases of pneumonia that have fallen under his care, in which nitrate of potash was beneficially employed. Everyone, he says, is familiar with the different plans of treatment and the particular remedies, such as tartar emetic, digitalis, quinine, veratria, alkalies, and chloroform, which have each had their special advocates; as also the various methods employed for combating the inflammatory process locally, as depletion, counter-irritation, cold, cataplasms, &c. Digitalis and quinine he regards as the two most powerful

antipyretics, we possess when given in sufficient quantities; the latter he has frequently given in doses of ten and fifteen grains every third hour without producing any unpleasant symptoms. Digitalis he considers to be particularly useful in the latter stages of the affection. Tartar emetic holds, however, the foremost place, he thinks, as an antipyretic in the treatment of pneumonia, though its administration sometimes requires to be stopped on account of its lowering and debilitating effects. Of late he has altogether relinquished its use, and has treated nearly every case of acute pneumonia which has come under his observation in *the earlier stages* of the disease with nitrate of potash in ten and fifteen grain doses, repeated every third hour, till it has produced its peculiar effect on the temperature and the pulse. In some cases, in consequence of complications, he has had to give up the nitrate and has resorted to quinine or digitalis. In regard to general measures he leans to a free and generous support, and rather to the side of the stimulant system than the reverse. He thinks counter-irritants and vesicants, followed by the application of large warm linseed-meal poultices, extremely valuable auxiliary measures. Dr. Jones then records several cases in which the above plan of treatment was successfully adopted. He thinks the mode in which it cures the inflammation or arrests its progress is partly by promoting the absorption of its products through its action on the fibrin, by a direct action on the blood through an effect upon its corpuscles, and also by reducing the force of the fever and lowering the force and frequency of the heart's pulsations. (*Dublin Med. Journal*, July 1873.)

Bromide of Potassium as a Febrifuge.—Dr. Maclean, of Applecross, records a case of a well-nourished woman, aged 40, who was attacked with enteric fever about the 6th of January. On the 10th, when first seen, the pulse was 140, the temperature $105^{\circ}2$ F. The respiration was hurried; there were sibilant râles in both lungs; countenance anxious. Tongue red at edges, with whitish fur down the middle. Bowels relaxed, with a little blood in the stools. There was tenderness on pressure in right iliac region. A dose of chalk mixture with ten minims of tincture of opium was ordered, to be followed by twenty grains of bromide of potassium, three times a day. On the 13th there was marked amendment in all the symptoms, which continued till the 17th, when the bromide was omitted. A relapse immediately occurred, but on the recommencement of the bromide, improvement was again observed, and by the end of the month the patient was fairly convalescent. Dr. Maclean suggests that the bromide acts by constricting or giving tone to the minute vessels directly, and is thus calculated to strike

at the very root of the disease in fever, since it takes the place of the paralysed vasomotor system of nerves, and thereby keeps in abeyance that inevitable tendency to congestion that follows the premonitory stage of the disease, the stage characterised by excitement or rigor. He is therefore of opinion that the use of the bromide in the treatment of pyrexia is much more in accordance with physiological principles than the direct application of cold; for however useful the treatment by cold water may be, one thing to be remembered is, that we are not treating *heat*, for temperature is a mere symptom of disease—a useful index, no doubt, of the degree of mischief and oxidation of the elements going on in the system. (*British Medical Journal*, July 5, 1873.)

Determination of the Quantity of Blood diffused in Urine in Hæmaturia.—Dr. Charles Ralfe, of the Seamen's Hospital, suggests the employment of a colorimetric method of analysis as furnishing a simple and ready means of ascertaining the quantity of blood present in urine. In order to obtain standards of colour, he first filled a number of tall cylindrical glass vessels capable of containing 100 centimetres with urine, and to each vessel he added a definite quantity of fresh uncoagulated blood. To the first vessel two minims, to the second four minims, to the third six minims, and so on. As 16·3 minims equal one cubic centimetre, it is obvious that the first vessel contained one part of blood in about 800 of urine, the second, one part in 400, and so on. The colours given when viewed by transmitted light on the addition of different quantities of blood to 100 c.c. of urine were then noted, and were found to be as follows:—(a) With urine containing two, three, and four minims, the colour imparted was a brown sepia or *smoky* tint. This tint was even perceptible when only one minim was added, or one part in 1,600. (b) In urine containing five, six, seven, and eight minims, the colour passed gradually from the smoky tint to *bright cherry red*. (c) In urine containing ten, twelve, to sixteen minims, the bright cherry red passed into a *dull maroon red colour*. (d) After the addition of sixteen minims the urine acquired a *deep chocolate brown colour*, and after twenty minims it was impossible to note any further change of colour. Having obtained four distinct colours by the addition of varying quantities of blood to urine, Dr. Ralfe had next to learn how far these artificial solutions corresponded with urines obtained from actual cases. For example, the urine obtained from a patient suffering from acute nephritis exhibited the smoky tint; compared with the artificial solutions it corresponded to one containing four minims of blood, or one part in 400. The two urines were then boiled, liquor potassæ added

and again boiled, the reddish brown precipitate consisting of phosphates and hæmatin removed by filtration and incinerated. The ashes were analysed for iron, and a close correspondence was found to exist between the real and the artificial solution. Other experiments with urines containing larger amounts of blood showed a similar correspondence. From these observations the following clinical facts may, Dr. Ralfe thinks, be deduced:—1. That a *smoky brown* colour is imparted to urine when blood is present in quantities from one part in 1,600 to one part in 400; that a *cherry red* colour is given when one part in 350 to 200 is present; that a *dull maroon red* is given when one part in 150 to 100 is present; whilst in more concentrated solutions the colour assumes a *deep chocolate brown*. 2. That the quantity of blood which gives to urine these distinct colours is much less than has been hitherto supposed. (*Lancet*, June 21, 1873.)

The administration of Perchloride of Iron.—Delicate patients, says Dr. Herbert Snow, very frequently object to the astringent metallic taste long remaining in the mouth after the administration of tincture of perchloride of iron, the flavour of which is but very imperfectly disguised by the syrup or spirit of chloroform with which it is usually ordered. It is worth knowing that the substitution of a small quantity of glycerine (about half an ounce to an eight ounce mixture) will altogether obviate this inconvenience. (*British Med. Journ.*, June 28, 1873.)

Oxide of Zinc in Infantile Diarrhœa.—Dr. E. Mackey, of the Children's Hospital, Birmingham, expresses himself as being strongly in favour of the use of oxide of zinc in the diarrhœa of children, which he regards as preferable to chalk and kino. Oxide of bismuth has tonic and antispasmodic properties, a combination in a non-irritant substance exactly suited to many cases of the malady. Chalk is good, but sometimes irritates and sometimes fails. Acids are good, but sometimes gripe and sometimes injure teeth. Opium should be absolutely forbidden for infants that cannot be closely watched. Bismuth is very good, and zinc resembles it, with better nerve-tonic powers, whilst it is much less expensive. Oxide of zinc has given him (suitable diet being premised) excellent results in all the varieties of infantile diarrhœa, notably in those complicating whooping-cough; it is not to be forgotten in the profuse sweating of rachitis. The dose may be one grain for any age under two years, and may be well given with a little syrup, mucilage, and dill water, three or four times daily, not on an empty stomach.

In a short paper upon the diarrhœa of children, in the same journal as that from which the above quotation is made,

Dr. Eustace Smith remarks that teething infants are excessively sensitive to slight changes of temperature, and the protection of the body from impressions of cold should be the first precaution to be adopted in all cases of abdominal derangement in children. Dr. Smith states it has long been his practice to recommend the application to the belly of a flannel binder, which should be applied low down on the abdomen, and be firmly wrapped round the hips and buttocks, and should be broad enough to cover the body as high as the waist. This, with a dose of castor-oil to remove irritating matters from the bowels, will ease at once almost all cases of acute functional diarrhoea in children. In all cases when the child is not at the breast, milk should be excluded for a day or two from the diet. Its place can be supplied by whey, veal broth, and barley-water in equal proportions. (*Ibid.* July 12, 1873.)

Stricture of Œsophagus treated with Bromide of Potassium.—Mr. James Allen records a case occurring in the Belford Hospital, Fort William, of a man, aged 66, who was suffering from inability to swallow. The food returned and seemed to stick about the epigastrium. There was pain in that region when food was taken, but nowhere else. The difficulty of swallowing had commenced about a year previously; he had never vomited blood. On inspecting the chest there seemed to be a degree of bulging of left front; strong pulsation was observed between the xiphoid cartilage and the lower border of ribs on the left side; radial pulses seemed equal in force; pupils were equal; a rough systolic bruit was heard over apex of heart. Pulse 72, tongue clean. Three years ago had an epithelial cancer removed from lower lip, which had never returned. He was advised to apply a belladonna plaster over the epigastrium, and to take the following mixture:—Ten grains of bromide of potassium, with eight drops of the tincture of the muriate of iron, three times a day. Within a week he had so far improved as to be able to swallow some food. Within a fortnight the vomiting had ceased, but the patient still complained of some pain and a feeling of constriction, and said that "the road was not very clear yet." Within three weeks he was able to swallow any kind of food, but a little pain was still felt. No bougies were passed, since it was thought probable from his age and previous history that the disease was malignant. The improvement was, if it prove only temporary, at least well marked whilst he was taking the medicine. (*Lancet*, July 12, 1873.)

Treatment of Chronic Inflammatory Diseases of the Larynx.—After pointing out some of the advantages of laryngoscopy and describing the instruments required, Dr. Sawyer observes that chronic laryngitis is most commonly met with in males;

the patient complains of tickling, the voice is weak, harsh, and hoarse, cough is slight, and there may be a little pain in swallowing. On examination the mucous membrane is found reddened and thickened, with a few patches of adherent mucus, the vocal cords being especially hyperæmic; ulceration is rarely seen. The disease is common in those who use the voice much, and in those who are suffering from chronic pulmonary phthisis and from syphilitic disease. In regard to treatment, gargles are useless, since they rarely reach beyond the posterior pillars of the fauces. A solution of the nitrate of silver, sixty grains to the ounce, or of chloride of zinc, fifteen grains to the ounce, or of sulphate of copper of the same strength, or, lastly, of perchloride of iron, sixty grains to the ounce, may be applied with the laryngeal brush. The nitrate of silver is preferable, though it produces more smarting and uneasiness than the chloride of zinc. At first, until very decided improvement is manifested, the applications should be made daily, afterwards every other day, and then once or twice a week until the cure is effected. Chronic glandular laryngitis is a tedious affection, but we may do much good by restricting vocal exertion, and applying the nitrate of silver or the chloride of iron. In phthisical laryngitis the nitrate of silver is the best application. The laryngeal affection in this disease can only be alleviated, not cured. In the treatment of syphilitic disease the constitutional treatment should not be forgotten. Much benefit may be derived from inhalations. Medicated steam inhalations are of great value. (*British Medical Journal*, July 12, 1873.)

Extracts from British and Foreign Journals.

Transfusion of Blood.—Dr. Kirchner, of St. Louis, gives a translation of a paper by Dr. H. Leisrink, of Hamburg (published in Volkmann's Clinical Lectures) in the *St. Louis Medical and Surgical Journal*. The first recorded case on which reliance can be placed, in which transfusion of blood was performed, is that mentioned by Lower in 1666, and it was frequently repeated in animals with varying success, but it fell into disrepute, partly from religious scruples, partly from the failures, or rather from the too high anticipations, that were made of it. James Blandell, in 1824, operated successfully in man, on four occasions rapidly succeeding each other. Dumas, Dieffenbach, and others adopted it, and Panum pointed out that in all instances human blood should be used, and that this should be defibrinated. Hüter, who suggested arterial transfusion; Martin, who used it especially in parturient cases; Kühne, who employed it in poisoning by oxide of carbon; Neudörfers, who practised it in chronic anæmia consequent upon purulent discharges; and Nussbaum, who performed it in chlorosis, have all added to our knowledge of the subject.

The indication for transfusion may be formulated thus:—

Transfusion is indicated in all those pathological conditions where all the blood, in quantity and quality, is so altered that it is unfit to fulfil its physiological duties. In acute anæmia we have the most brilliant aspect of the operation, and this is especially apparent in the hæmorrhages of parturient or lying-in women, and in those that occur after operation or injuries. It would be obviously unserviceable in the losses of blood occurring in consumption, in dysentery, and similar diseases. It may be considered as demonstrated that the injection of blood is the best stimulus for the relaxed uterus. Esmarch made transfusion on a person during the extirpation of a fibro-cancerous tumour from the base of the skull, where much blood was lost, by pumping back new blood into the arm; and in a case of exarticulation of the femur, he caught up the blood and injected it directly into the femoral vein. In chronic anæmia it is expedient to make

frequent small transfusions in preference to a single large one' to accustom the weakened heart and poorly nourished tissues to the presence of the new fluid. Defibrinated blood should be used, partly to do away with practical difficulties in the process of transfusion, and partly because it makes venous blood arterial. It may be defibrinated by beating it with a clean wooden stick, and straining in a vessel floating in water contained in another vessel at 100° Fahr. It is not necessary that the syringe should be frightfully complicated; any syringe will do, taking care to avoid the introduction of air; but Uterhart's or Ulenberg's are the best. Venous transfusion is a more simple operation, and answers the same purpose as the arterial. "Suppose we are called upon to make transfusion. A healthy, robust man, who is willing to give the requisite supply of blood, is at hand: we draw blood from a vein of his arm and catch it in a clean vessel, and defibrinate it in the above stated manner. Is it ready, you must keep it warm while you turn to the patient; you look for a favourably situated vein, say the median basilic or cephalic, which is laid bare by an incision. With a few more cuts you prepare it free in its circumference: the distal end is now ligated, so as to keep the field of operation free from blood. A little above you place a second ligature, but without tightening it. You now fill a Uterhart's syringe and also its canula, and carry the latter through the incision into the vein. To the canula you attach the syringe, and drive, with a slow rotary movement of the piston, the blood into the vein. Some person you will find to whom you can entrust the pulse of the other side, and who counts its beats aloud. In doing this we are secure against accidents, and you can see the breathing of the *not chloroformed* patient during the injection yourself." A slight deviation of the pulse always occurs; it either becomes more full, or it becomes for a moment smaller. This need not disturb the operator, nor need a slight cough, or disquietude of the patient. Strong oppression of the chest, irregularity of the pulse, demand caution, but syncope demands discontinuance of the operation. You will certainly guard against some of the eventualities by quietly talking to the patient, for the moral impression of the operation is a great one. Accidents demanding a discontinuance of the operation are very rare. (*St. Louis Medical and Surgical Journal*, No. 3, 1873.)

Diagnosis of Aneurism from Abscess.—Dr. Stephen Smith, of New York, gives the following as the scientific tests employed for the purpose of diagnosing aneurism:—1. A tumour in the course of an artery. 2. Pulsation synchronous with the heart, and of an expansive character. 3. Cessation of pulsation when the artery is compressed on the cardiac side with partial

subsidence of the swelling. 4. A bruit heard with the pulsation. 5. Exploration. In regard to these several points he remarks:—(1.) That a tumour situated in the course of an artery is of frequent occurrence without the presence of aneurism, so that in itself the symptom is of no positive value. (2.) Pulsation may or may not be present when aneurism exists; to be of value when aneurism exists it must be expansive; and yet expansive pulsation may be present in an abscess surrounding an artery, and no aneurism be present. (3.) Cessation of pulsation will occur in any tumour or swelling situated over an artery when compression is made on its cardiac side; subsidence of the swelling on pressure may not occur in aneurism, and may occur in abscess. (4.) A bruit may or may not be present in aneurism; when present, it is variable in character; it may be present when any tumour or abscess is situated over an artery. (5.) An exploratory puncture may fail to give exit to blood in an aneurism, and may give a jet of blood in various kinds of tumour. From all of which it appears that the several scientific tests or symptoms of aneurism are extremely variable and of doubtful value when present. The various phases of growth of an aneurism may assume all the most marked features of an abscess and lose its own characteristic symptoms. Thus an aneurism may form without pulsation or a bruit, and may have heat, pain, and fluctuation; while an abscess may form without pulsation and a bruit with an inconsiderable amount of heat and pain. Again, an aneurism may form with an abscess overlying it, and the symptoms of the two may become inextricably intermixed. Finally, an abscess may form which subsequently opens into an artery, and thus suddenly assumes all the apparent conditions of an aneurism. Dr. Smith then proceeds to give a series of very interesting cases culled from recent medical literature, in which many of the difficulties above alluded to in establishing a correct diagnosis were present. (*American Journal of Medical Science*, April 1873.)

Strychnia in Nervous Affections.—Dr Chisolm relates the results of his experiments on the effects of the hypodermic injection of strychnia in cases of optic nerve atrophy. He commenced with very small doses, not more than $\frac{1}{16}$ of a grain being injected each time, gradually increasing the quantity as he found the system tolerated it. The toleration he found as a rule was rapidly established, so that a dose which would cause decidedly uncomfortable contraction in the spinal and leg muscles, would, after a few repetitions, cease to annoy. To obtain all the good which strychnia can produce, he thinks it necessary to keep the system under the full physiological effects

of the remedy, by administering a dose as large as the patient can bear comfortably—as a rule, half a grain, in divided doses, can be comfortably borne by most persons. Comparative experiments were made to ascertain whether equal doses could be taken by the stomach, and in the course of these it was discovered that while the same dose injected hypodermically, and ingested into the stomach, acted rather more promptly in the former case, the same dose was not equally well borne at all times of the day. A larger dose could be taken after eating than after fasting. The dose in the morning, again, may be larger than that at mid-day, and this again than in the evening. Its action is to some extent cumulative. It enters the system with much greater rapidity than it can be eliminated. Finding that large doses of strychnia when taken by the mouth will produce the identical immediate and final results as when injected under the skin, Dr. Chisolm has abandoned the hypodermic use of the remedy. Its intense bitterness he avoids by giving it in the form of sugar-coated granules. These, he thinks, act quite as well as the liquid preparations, and, being more definite, are safer. He uses granules of $\frac{3}{30}$, $\frac{1}{20}$, $\frac{1}{15}$, and $\frac{1}{10}$ of a grain of strychnia in each, commencing with the feeble doses, and in the course of a fortnight or three weeks running up to the higher doses. The evening doses should be light. Ultimately about half a grain of strychnia may be given per diem, which is the dose that produces the best remedial effects, and this dose may be continued for months. (*Hay's Amer. Journ. of Med. Science*, April 1873.)

Treatment of Spermatorrhœa.—In a clinical lecture delivered by M. Maliez upon this subject, and reported by M. Jardin in the *Mouvement Médical*, he observes that the condition of the spermatic ducts and passages should be first carefully investigated, in order that the slightest departure from a healthy condition may be observed and treated; the volume of the prostate should be carefully noted, the anus and the rectum should be explored, to ascertain the presence or absence of fissures, worms, &c. Attention should also be paid to the state of the bowels, daily evacuation of their contents being insisted on, and with facility; care should be taken to discover, by direct inspection, whether the proportion of prostatic fluid does not form by far the larger part of the fluid emitted. To ascertain the presence of spermatozoa and the degree of their vitality, minute interrogatories should be made as to the habits of thought of the patient, his mode of life, and the causes which may have predisposed him to the disease. The strength of the patient should be judged of by his mode of walking and by the dynamometer, and it should be found out whether the emission occurs only during sleep, during violent exertion, or during mic-

turition. M. Mallez regards the alkaline bromides as occupying the first position in point of importance in the treatment of this affection, since they have a double action both upon the spinal irritation and on the local sensibility. The preparation he prefers is that suggested by M. Pelisse, namely, desiccated bromide of potassium, an ounce; tolu water, ten ounces: of this, four dessert spoonfuls, containing about a drachm of the salt, can be taken per diem. The administration of the bromide should precede any local treatment, and may be continued from eight days to two months without harm. After the lapse of ten or twelve days, continuous currents should be applied, though there is some difference of opinion in regard to the manner in which they should be applied. M. Mallez himself prefers to make use of descending currents, passing down the whole length of the spinal cord, from the occipital to the lumbar region: the source of the electricity being from eight to ten elements of a Gaiffe's pile, with chloride of silver. After using this for eight or ten days, the direction of the current may with advantage be reversed. Valerianates of ammonia of zinc and quinine, M. Mallez has found of less service than the foregoing plan. The application of cold water douches to the belly should not be indiscriminately recommended, as they occasionally seem to excite rather than to repress the discharges. (*Le Mouvement Médical*, June 14, 1873.)

The Ætiology and Treatment of Chronic Inguinal Adenitis.—Dr. Marcano observes that inguinal adenitis, being usually only the symptom of some other disease, its character varies according to its cause. The first thing that suggests itself to the surgeon, under ordinary circumstances, is that he has to deal with a bubo, and he looks for the chancre which has led to its formation. If this be absent, he seeks other causes. But the existence of the bubo being established, it is still of importance to determine whether the precedent chancre was simple or infecting. Sometimes both conditions may occur, one supervening upon the other. Thus a man is attacked with a soft chancre, which is accompanied by the formation of a bubo on the right side. This disappears, but leaves a certain degree of induration behind it. Some time afterwards a second adenitis occurs on the same side, but this time caused by an indurated chancre. This also leaves an induration, which is superadded to the foregoing. Lastly, the patient may contract gonorrhœa, which once more causes adenitis on the same side. The tableau may be completed by considering him to be of a strumous constitution and exhausted by sexual excesses or liable to great exertion and fatigue. If the skin be cut through which covers a tumour, developed after these attacks, a discharge of pus may

or may not take place; in either case, however, a reddish tissue is exposed, very friable, formed by the connective tissue around the ganglion. In the centre of this substance ganglia appear in the form of round bodies with irregular surface of various size, but always smaller than the original gland. When suppuration exists, its seat is in the cellular tissue. If the gland be divided, a little drop of pus occupies its centre. By various methods of treatment, the aspect of the wound can be modified, the suppuration can be arrested, and the skin may even be made to cicatrize, yet the patient is far from being cured. Mere fatigue will bring on a fresh inflammatory attack. In order to effect a cure, it is absolutely necessary that the gland should be abolished, since its presence is the condition which occasions suppuration. The various processes by which it has been attacked have led to a hypertrophic condition, and it really plays the part of a foreign body, provoking irritation in the adjoining tissues. If we examine now the various plans of treatment that have been suggested, we shall find that they all either endeavour to prevent the formation of pus, or to evacuate it as soon as it has formed, and they are successful enough if the bubo is acute, but they fail in a case of chronic adenitis, because there are other indications to fulfil. Extirpation of the glands is, indeed, the only means of effecting a cure. But everyone knows how dangerous and difficult it is to remove such glands with a bistoury. In a case not long since under the care of Dr. Demarquay a lymphosarcoma was thus taken away: erysipelas followed, and death. Other means must therefore be looked for, and M. Demarquay has adopted certain caustics. His plan of procedure is to make a cut through the skin over the middle of the tumour, of sufficient length to enable the whole of the indurated parts to be exposed. Two or three days after he introduces some arrows, made of the *paté de Canquoin*, into and beneath each gland. About the third or fourth day, according to the size of the tumour, the caustic is removed if it have not fallen out spontaneously; if any glandular indurations remain, the process is repeated till all are removed. The wound then enters into the condition of ordinary wounds, and cicatrization proceeds rapidly, after which the patient is cured. It is said that the chloride of zinc and other caustics leave an ill-formed cicatrix; but if care be taken that it does not act on the skin, only the line of incision made by the bistoury will be left. This mode of applying caustic is quite different from that of Melchior Robert, who coated the glands with Vienna paste, from that of Pirondi, who applied tincture of iodine, and from that of M. Malapert, who applied corrosive sublimate. M. Rollet opens the skin over the bubo with Vienna paste, and then applies the chloride of zinc for half an hour, or even for four or five hours,

but only with the object of modifying the aspect of the wound; that is to say, the *paté de Canquoin* is not permitted to destroy the gland. The point of importance is not so much, however, the particular caustic employed, but the principle that the gland must be removed if a chronic adenitis is to be cured. The chief objection to the use of caustics is the pain they produce, but this appears to differ considerably in different cases, and not to be sufficient to deter either the surgeon from recommending, or the patient from undergoing, their action. (*Bulletin Général de Thérapeutique*, 10^{me} livraison, 1873.)

Varicose Hæmorrhage from the Cervical Zone of the Uterus, complicating Labour.—Dr. Murray draws attention to a remarkable form of hæmorrhage that occurred in one of his patients, of which the following is a history:—Mrs. B., aged 27, first labour, full term of gestation: in a weak state of health and of a nervous temperament. She was about to retire to bed, when she felt blood running from her. She sent for a physician—Dr. Kirby—who plugged the vagina, and all bleeding ceased. The os uteri was then only partially dilated, and the liquor amnii had not escaped. Dr. Murray was then called in to consultation. Dr. Murray removed the plugs, and found that the os uteri had become fully dilated, that the membranes were tense, and that very little hæmorrhage was going on. No portion of the placenta could be felt. A full dose of ergot was given, the membranes were ruptured, and the labour progressed quickly, and terminated favourably. Thirteen months after the patient was seen by Dr. Murray, at Dr. Kirby's request, in her second labour. Hæmorrhage had again set in as before, without appreciable pain. Dr. Kirby had plugged the vagina, but a small stream of dark blood escaped continuously. On removing the plugs, Dr. Murray found the os uteri dilated to the size of a crown, the membranes unruptured, and the foetal head presenting. The cervix felt large and pulpy at its posterior aspect, and was rather low down in the vagina. No trace of the placenta could be felt on the most careful examination. The pulpy part yielded on pressure with the fingers, and coincidentally the bleeding diminished. The same treatment as before was adopted, with the same results. Cases somewhat similar to these have been described by McClinton and Mr. Robertson of Manchester, but their issue was less satisfactory, both patients dying. (*Obstetrical Journal*, No. 1, 1873.)

Fracture of the Clavicle treated by placing the Arm behind the Back.—A patient was recently under M. Broca's care, who had fractured his left clavicle by a fall, near the middle of the bone. The fracture was oblique, from above downwards and from without inwards, the fragments riding

over one another to a considerable extent. Various plans of treatment were tried, but without effecting permanent reduction into a good position. At length, calling to mind a communication made last year by Dr. Michel to the Société de Chirurgie, M. Broca placed the limb in a semiflexed position behind the back, when the most perfect confluence of the fragments occurred. The arm was fixed in this position by a bandage, and kept in it for eighteen days. At the expiration of this time the bandage was removed and the arm set at liberty. When it was found that the parts were sufficiently consolidated to prevent any likelihood of displacement, the limb was brought forward and kept immovable in a sling for a few days longer. This method of treatment has been regarded as excessively painful, but in this instance the patient only complained of the inconvenience and pain for the first twenty-four hours. At the same time it must be stated that he was a man of considerable nerve. Immediately after his entrance into the hospital, he several times raised the hand to the head, giving a fresh demonstration of the possibility of movement with fractured clavicle. The result was so good, says M. Broca, that had the patient been a lady, she might have worn a low dress without any disfigurement being observable. The proceeding of placing the hand behind the back in the treatment of fractured clavicle is not quite new, for M. Grout is cited by M. Malgaigne as having adopted it. M. Broca does not think this plan is applicable to all cases, since it compels the patient to sleep on the opposite side; but he agrees with Malgaigne in believing that in some fractures of the clavicle, the broken ends of the bone can only be brought into apposition by placing the upper extremity in special and peculiar positions, which may be quite different in different instances. (*Journal de Médecine*, tome xlv. 5^{ème} Cahier.)

Treatment of Varicocele.—Dr. J. F. Heustis, of Mobile, Alabama, thus describes a new mode he has invented for the cure or relief of varicocele. On the day previous to the operation the bowels should be thoroughly cleared out with a dose of compound cathartic pills, in order that the patient may not be compelled to leave the recumbent position for three days. The patient standing (to fill the veins with blood and render them conspicuous), the distended vein is carefully separated from the other constituents of the cord, and held between the left forefinger and thumb, their points pressed firmly and closely behind it, while a large *sewing* needle threaded with double silk thread, leaving a loop, is quickly passed through the scrotum behind the vein close to the ends of the fingers. A piece of fine pure silver wire is bent on the loop of thread, and drawn through. The patient now lies down, and the vein becomes empty. The

skin of the scrotum in front of the vein is pinched up and drawn away from the vein, and a blunt-pointed sewing needle, threaded with the double silk with the wire bent on, is passed through the opening made by the first, in front of the vein, entering at the opening of exit, and emerging at the opening of entrance of the first; the point of the needle being directed towards the front part of the scrotum as it passes through from one opening to the other, while the pinched-up skin is kept drawn forwards. This ensures the closure of the vein (and nothing else) in the loop of wire. The two ends of the wire are passed together through a leaden button and an opening in one end of a piece of vulcanised india-rubber ($\frac{3}{16}$ of an inch thick, $\frac{3}{4}$ in. wide, and $2\frac{1}{2}$ in. long). The other end of the india-rubber is bent down, and the wires passed through separate openings in it, and loosely tied with a single knot. The operation is now suspended to bring the patient fully under the influence of chloroform, as the tightening of the vein is very painful. The rubber is now pressed down on the button and scrotum, and the wire drawn firmly through and tied or twisted. A thin wire previously holding the ends of the wire together is now snipped, and the two ends of the rubber spring apart, increasing the tension of the wire. The piece of snipped wire is removed, the ends of the ligature cut off, and the operation is finished, and, if neatly done, is almost if not quite bloodless. Morphia is given if the pain becomes severe, and the patient should use the bedpan to pass his water for three days, so as not to refill the ligatured vein by rising from the recumbent posture. At the end of that time the wire is snipped and withdrawn, chloroform being administered to prevent pain, but the patient should keep his bed for four days longer, to allow the inflammatory swelling to subside. In ten days he can return to his business. The swelling around the vein at the point of ligature is at no time very considerable, and after the removal of the wire subsides rapidly, scarcely leaving a trace of the operation in a few weeks. Whilst the swelling lasts the testicles should be supported with a suspensory net, but there is no need of it after all thickening has disappeared. During the inflammatory action immediately following the operation, the testicles are supported with a linen handkerchief or towel passed behind them, its ends fastened to another round the waist, and lead-water and laudanum kept applied on lint. (*New York Medical Journal*, vol. xvii., No. 4, 1873.)

Treatment of Snakebite by Artificial Respiration.—Mr. Vincent Richards, in an article contributed to a recent number of the *Indian Medical Gazette*, remarks that when snake-poison is injected into the areolar tissue absorption gradually takes

place. When the poison reaches the lungs it appears to excite the vagus, and, through the medulla and spine, the phrenic and intercostal nerves, leading at first to an accelerated action of the respiratory muscles, and afterwards, as a larger quantity of poison becomes circulated through the lungs and the stimulus to the nerve-centres is augmented to paralysis of them, vomiting, which is a frequent though not constant symptom, probably arises from this irritation of the vagi. The medulla oblongata and spine are indeed primarily affected, and it is only as a secondary effect that the cerebral ganglia and cerebrum are involved. Presuming this to be the physiological action of the poison, it follows that a person fatally bitten dies from asphyxia produced by paralysis of the motor respiratory nerves. The indication of cure, provided the effect of the poison on the nerve-centres is not permanent, is therefore artificial respiration. Moreover, if elimination of the absorbed poison can go on, as appears to be the case, we have good reason to hope for favourable results. When, however, the amount of poison injected is overwhelming, Mr. Vincent Richards thinks little can be hoped for from the treatment. That an animal may be affected even to convulsions he has shown in Fayrer's "Thanatophidia," and he believes that when the quantity of poison injected into the areolar tissue would under ordinary circumstances be just sufficient to kill, artificial respiration, if properly maintained, might save life as it does in the case of curara poisoning. It was this latter fact which first led Dr. Fayrer to make a trial of artificial respiration in snake-poisoning, and the subsequent encouraging results which he obtained induced him to suggest its adoption in such cases. In one of Mr. Richards' cases, where artificial respiration was adopted, the heart's action was maintained in a dog for ten hours, and then ceased only on the continuance of the artificial respiration; and in another the heart was kept beating for twenty-four hours and thirty-five minutes, sensibility being restored after it had been completely lost. The results, as shown by these experiments, are therefore very encouraging. The advisability of applying a ligature and cauterising the bitten part is suggested by one of the experiments. (*Indian Med. Gazette*, May 1873.)

Amputation of the Penis by the Galvano-caustic Method.—In the last part of the Italian journal *L'Imparziale*, Dr. Pisa gives an interesting account of the facility with which the galvano-caustic method can be applied to the amputation of the penis in cases of cancer and canceroid. In the case he reports the patient was admitted into the Royal Hospital of S. M. Nuova Gaetano Nesi di Greve, under the care of Dr. Frascani. He had enjoyed good health up to about a year previously, when a swelling appeared at the corona glandis, which

gradually advanced, invading the glands on the one side and the prepuce on the other. In the early stages of the affection no pain had been experienced, but latterly he had had some lancinating pains, and the tumour had assumed a fungous aspect, and discharged pus. At a consultation held between Professors Corradi, Nuici, and Gozzini, it was decided that the tumour with the extremity of the penis should be removed by the galvano-caustic method. Accordingly a platinum thread was passed round the organ below the tumour, and heated to whiteness by the electric current. The gland and part of the prepuce affected were removed in the short space of thirty seconds, the patient scarcely suffering any pain, and a healthy surface being left. Scarcely any hæmorrhage occurred. A gum elastic syringe was introduced into the urethra. Everything went on well for nine days, when, unfortunately for him, two cases of hospital gangrene occurred in the same ward. He was attacked with febrile symptoms, became jaundiced, and died on the twentieth day after the operation. (*L'Imparziale*, June 16, 1873.)

The Forms and Œtiology of Diarrhœa in Children.—

In a paper contained in the *Jahrbuch für Kinderheilkunde*, Band. vi. p. 1, Dr. Widerhofer differentiates three chief forms of diarrhœa in children:—1. Diarrhœa from dyspepsia originating in the stomach and arising from the irritation produced by imperfectly digesting milk. 2. Diarrhœa from intestinal catarrh, the principal part affected being the small intestine, and caused by increased intestinal secretion. 3. Diarrhœa from enteritis, with inflammation of the large intestine. To these forms may be added choleraic diarrhœa resulting from general disorder of the system.

1. In *Diarrhœa from Dyspepsia*, the chief constituent of the stools is fæculent matter, the matter discharged being pultaceous, of unequal consistence and colour, so as to resemble minced egg. The smell is acid, and after a little while resembles bad eggs. The reaction is acid. The child suffers from colicky pains, eructs wind with the odour of sulphuretted hydrogen, and the belly is swollen with wind. There is no fever, except that the pulse is a little increased in frequency in the evening. The temperature of the skin and the cutaneous transpiration alter only during the attacks of colic. There is no thirst, and the secretion of urine is normal. Nutrition is only interfered with when the affection is of long standing. Dyspepsia may pass into other forms of diarrhœa, and especially into enteric catarrh. Rachitis and anæmia are frequent results of dyspepsia.

2. *Diarrhœa from Enteric Catarrh*.—Water is the chief constituent of the stools, hence their fluidity. The colour is a uniform brown or green, the smell is penetrating, and in the

later stages resembles sulphuretted hydrogen. Reaction at first acid, subsequently alkaline. The passage of the motions is accompanied by colicky pains and explosions of wind; the belly is tympanitic. At first there are sharp febrile symptoms, but these are only transient. There is much thirst. The urine is diminished in quantity. Considerable loss of flesh occurs in the course of two or three weeks. Sometimes blood and mucus appear in the evacuations, and then the previously moist tongue becomes red, dry, and shining: the enteric catarrh has then passed into enteritis.

3. *Diarrhœa from Enteritis*.—The chief constituent of the motions is mucus, which is gelatinous and tenacious, resembling the envelope of the eggs of the frog, and it is mingled with blood or pus. Smell, either none or resembling putrid meat; reaction indeterminate. Pain is experienced in the abdomen. The call to evacuate the bowels is frequently repeated, but the quantity of matter passed with each motion small. Belly usually soft, rarely tense; not much flatus. Thirst increased and urinary secretion diminished. A little fever is commonly present. The general nutrition of the body soon becomes impaired. The affection lasts from one to six weeks. It may pass into cholera infantum; hydrops meningialis; ulceration of the intestines and anæmia.

4. *Diarrhœa Cholericiformis*.—The chief constituents of the fecal evacuations are water and epithelium. The motions are extremely fluid, and in severe cases quite colourless. There is frequent vomiting. The smell of the stools is more or less ammoniacal, and the reaction alkaline. The belly is soft, doughy, and there is no escape of gases. The general symptoms of cholera are present. It lasts only a few days, and death occurs from collapse. In regard to the relations of this division of diarrhœa to the older divisions, Dr. Widerhofer observes that formerly the diarrhœa infantum was looked upon as similar to that of adults, but modified by the peculiarities of the infantile organisation, and it was divided in the following mode, according to its real or supposed causes:—*Diarrhœa ex dentitione*; *D. ex acidis*; *D. gastrici*, and *D. ex verminosi*. Trousseau divided diarrhœa in accordance with the appearance of the stools into *D. biliosa*, *mucosa*, *lienterica*, *cholericiformis*. Certain kinds of diarrhœa, as, for example, diarrhœa from dentition, was regarded as a natural curative means, or as warding off other affections, but the school of Vienna consider diarrhœa to be always a pathological process. Diarrhœa from the teeth serves conveniently to explain many cases of which otherwise no account can be given. It is possible that the bowels may be deranged by the largely increased quantity of saliva that is swallowed, especially if stomatitis be present, but it is impossible to explain the diarrhœa

rationally by supposing it to be due to reflex nervous action. There are no characteristic discharges in the diarrhoea from the eruption of teeth, nor are there any proper pathological processes belonging to it. The diarrhoea from worms has an equally unfounded scientific basis. The presence of a large number of worms may certainly increase the secretion of mucus, and cause the evacuation of such mucus, but such diarrhoea is usually attributable to other forms. Diarrhoea ex acidis is that form of dyspepsia which is caused by excess of acid in the stomach. Diarrhoea biliosa is the catarrhal form. Diarrhoea mucosa corresponds to the enteric form, &c. &c. A form of diarrhoea sometimes occurs when children are weaned. This form is characterised by its great obstinacy and by the readiness with which it passes into conditions which are dangerous to life. Primarily it belongs to the dyspeptic form of diarrhoea, but during the hot season of the year it is apt to run into the choleraic form. Dr. Widerhofer then proceeds to consider and define the diarrhoea of older children, and he recognises the following types:—1. Diarrhoea ex gastro and ex entero-catarrho. 2. Ex enteritide. 3. Ex dysenteria. 4. Ex cholera Asiatica. 5. Ex typho; and 6. Ex tuberculosi. (*Medizinisch-chirurg. Rundschau*, June 1873.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

WHITBREAD AND CO.'S BOTTLED ALES AND STOUT.—Whitbread and Co.'s ales and stout have long enjoyed a considerable amount of popularity, a popularity which a careful chemical examination enables us to state is thoroughly well deserved. The three samples analysed, two of bottled ale and one of bottled stout, were all of high quality, being thoroughly well fermented, perfectly sound, containing indeed a remarkably small percentage of acid, of good alcoholic strength, and with a fair amount of extractives. All this proves not only that the full proportion of malt has been used, but also that the various operations of brewing, &c. have been conducted with skill and care.

Some of the analytical details are given in the table below.

100 parts by weight contained	Strong Ale.	Ale.	Stout.
Alcohol	6·65	4·94	5·07
Dextrin	5·56	2·09	2·99
Sugar and other extractives	1·80	1·96	3·29
Mineral matters	0·44	0·35	0·38
Water	85·55	90·66	88·27
	100·00	100·00	100·00
Acetic acid	0·015	0·024	0·072
Lactic and other fixed acids	0·117	0·066	0·102
Chloride of Sodium ¹	0·056	0·034	0·021

¹ Or, per gallon, 39·2, 22·8, and 14·7 grains respectively.

CORRESPONDENCE.

DR. SHARP'S VINDICATION.—We have received from Dr. Sharp the following references to his own works which he considers as vindicating him from the insinuation in Dr. Ross's recent papers :—

“ *RUGBY*, July 8, 1873.

“ The Essays are at present out of print, but it is hoped that in a few months a new edition will be ready.

"The one referred to by Dr. Ross is on the Principle of Homœopathy, and treats of the following topics:—

"1. Whether there is any probability that a law, rule, or principle exists in nature for our guidance in the treatment of disease.

"2. The *law* of Homœopathy.

"3. The *limits* of this law.

"4. What those cases are which are *beyond the limits* of the law, and how they are to be treated.

"The extract made by Dr. Ross is from the last part. It is shown, in this part, that the cases, or rather the *parts of cases*, which are outside the limits of the law are mainly mechanical complications, such as fractures, &c., which, of course, require to be met by mechanical means of relief. But as Dr. Ross mentions 'dropsy' among these out-lying cases, perhaps one brief extract may be allowed to explain what is meant by this:—

"Again, cases of dropsical effusion *may* demand the removal of the accumulated water, not as a remedy for the dropsy, but that the distress caused by its bulk and *mechanical* pressure may, for a time at least, be relieved."

CHOICE OF ELECTRICAL APPARATUS.—In answer to a number of queries which we have received as to the comparative merits of the electrical instruments of different makers, we are obliged to make this general statement:—We cannot answer these queries individually, as there would be considerable risk of our being placed in the invidious position of appearing to favour particular makers. Whenever a new apparatus, electrical or other, seems worthy of examination, we shall report on it officially in our "Department of Analysis and Inventions," and such reports must be taken to be the expression of the best information and judgment on the matter at which we have been able to arrive.

HOMŒOPATHIC PILULES.—We have received from Mr. Frederick Ross (Ross and Leath, Vere Street, Cavendish Square) a protest against our analysis of his (and other) homœopathic pilules, or rather against some of the conclusions we drew from them. He tells us, what we confess we did not know, that the pilules do not contain $\frac{1}{100}$, $\frac{1}{10000}$, and so on, of a grain of the *drug itself* (e. g. belladonna, or nux vomica), but are moistened each with $\frac{1}{100}$, $\frac{1}{10000}$, &c., of a drop of a mother tincture which itself contains 10 per cent. of the drug. In our innocence we certainly had imagined that all "first dilutions" contained $\frac{1}{100}$ part, and all "second dilutions" $\frac{1}{10000}$ part, of the particular drug itself. Consequently we were not a little astonished to find, in the pilules of these dilutions, made by druggists of high respectability, no

traces of the alkaloids, even by the finest chemical tests. It now appears more certain than ever that the whole of the supposed therapeutic effects, even of first and second dilution pilules, must be the work of the imagination. No one who is not a mere advocate will seriously pretend that a dose of $\frac{1}{100000}$ of a grain of belladonna, representing 000000·3 grain atropine, will produce any real physical effect on the organism. We are very pleased to think that the effect of our published analysis has been to bring into prominence the monstrous character of the assumptions involved in the belief that any homœopathic pilules whatever can produce a true therapeutic effect. But it makes it more difficult than ever to imagine what can be the state of mind of those men who can calmly allege that the 200th *centesimal dilution* of such a dose—a dose which already eludes the most subtle chemical analysis—is also truly effective upon the body. Such men really exist, even in considerable numbers, especially in America; and we should like to know how Dr. Sharp, or any other of the higher intelligences among the homœopathists, explains the fact.

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¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

Department of Public Health.

THE MEDICAL OFFICER OF HEALTH.—HIS STATUTORY DUTIES.

UNDER the term *statutory* we include those duties of a Medical Officer of Health, as to certifying and advising, which he is called upon to perform under the Sanitary Acts. A review of these duties will, perhaps, most clearly show the subjects to which the chief responsibilities of his office attach, and with which he will find it requisite to be most thoroughly prepared to deal. We propose to recapitulate these duties in the order in which they appear in the Sanitary Acts, as cited in the Public Health Act, 1872.

1. *The Local Government Acts.*—These Acts include the Public Health Act, 1848 (11 and 12 Vict. c. 63); the Local Government Act, 1858 (21 and 22 Vict. c. 98); the Local Government Act, 1858; Amendment Act, 1861 (24 and 25 Vict. c. 61); the Local Government Act Amendment Act, 1863 (26 Vict. c. 17), and any enactments amending the same.

One section only of the Public Health Act, 1848, refers to a specific duty which the Medical Officer of Health may be called upon to perform under its provisions. By the 60th section of this Act, he may have to certify that any house or part of a house is in such a filthy or unwholesome condition that the health of any person is affected or endangered thereby, or that whitewashing, cleansing, or purifying of any house or part thereof would tend to prevent or check infectious or contagious disease.

But in relation to the provisions of this Act, he may be called

upon to advise the Sanitary Authorities as to proper (in the sense of not unwholesome) situations for places of deposit of sewage, soil, dung, filth, ashes, and other rubbish (sec. 56); as to the proper situation for public water-closets, privies, or other like conveniences (sec. 57); as to offensive collections of drainage, filth, water, or other matters, in ditches, drains, or pools (sec. 58); as to the keeping of swine in improper situations, and as to accumulations of manure, dung, soil, or filth, or other offensive or noxious matter (sec. 59); as to food of man unfit for use (sec. 63), a duty specially recognised under the Regulations of the Local Government Board (sec. 8); and as to bye-laws for the regulation of slaughter-houses (sec. 62), and of common lodging-houses (sec. 66).

The Local Government Act, 1858, gives the Sanitary Authority power to make bye-laws (*a*) for the prevention of nuisances arising from swine, filth, dust, ashes, and rubbish within their district, or of the keeping of animals so as to be injurious to the public health (sec. 32); and (*b*) as to the sufficiency of the space about buildings to secure a free circulation of air, and with respect to the ventilation of buildings; with respect to the drainage of buildings, to water-closets, privies, ashpits, and cesspools in connection with buildings, and to the closing of buildings or parts of buildings unfit for human habitation, and to prohibition of their use for such habitation (sec. 34). On all these matters the Medical Officer of Health is required by the Regulations of the Local Government Board (sec. 5) to advise.

Further, in the same Act are incorporated from the Towns Improvement Clauses Act, 1847, certain provisions for the prevention of smoke and for the licensing and regulation of slaughter-houses. The Medical Officer of Health's duties as to smoke as a nuisance will be regulated by the provisions of the Sanitary Act, 1866, but he should make himself acquainted with the qualification as to the provisions for the prevention of smoke contained in the Local Government Act, 1858, sec. 45. In respect to slaughter-houses, one of the incorporated clauses provides that the Medical Officer of Health, or other officer appointed for the purpose, may at all reasonable times, with or without assistants, enter into and inspect any building or place whatsoever kept or used for the sale of butchers' meat or for

slaughtering cattle, and examine whether any cattle or the carcase of any such cattle is deposited there; and in case such officer shall find any cattle, or the carcase or part of the carcase of any beast, which appears unfit for the food of man, he may seize and carry the same before a justice, and such justice shall forthwith order the same to be further inspected and examined by competent persons.

2. *The Artisans' and Labourers' Dwellings Act.*—Under this title is meant the Act so named of 1868 (31 and 32 Vict. c. 130). This Act has for object to provide better dwellings for artisans and labourers; and in sec. 5 it is enacted that if in any place to which the Act applies the Officer of Health finds that any premises therein are in a condition or state injurious to health, so as to unfit them for human habitation, he shall report the same in manner provided. Sec. 4 of this Act was repealed by sec. 11 of the Public Health Act, 1872; and all powers and duties imposed on Officers of Health under the Act were directed to be performed by the Medical Officers of Health appointed under the Public Health Act, 1872, the Sanitary Acts, or any Local Act.

3. *The Bakehouse Regulation Act.*—Under this title is meant the Act so named of 1863 (26 and 27 Vict. c. 40). This Act limits the hours of young persons employed in bakehouses, and makes regulations with respect to cleanliness and ventilation. The Medical Officer of Health, by sec. 6 of the Act, may be required to inspect any bakehouse during the hours of baking, and examine whether it is or is not in conformity with the provisions of the Act.

4. *The Diseases Prevention Act.*—Under this designation are included the Diseases Prevention Act, 1855 (18 and 19 Vict. c. 116), and the amendments of this Act contained in an Act for the Removal of Nuisances and the Prevention of Diseases (23 and 24 Vict. c. 77, secs. 10 and 12). In rural districts the Sanitary Authority appointed under the Public Health Act, 1872, is to be the local authority for the purposes of this Act wherever it is in force; and by the Regulations (sec. 17) of the Local Government Board, the Medical Officer of Health, both in rural and urban districts, is required to observe the directions and regulations issued under that Act by the Local Government

Board, so far as the same relate to or concern his office. The directions and regulations which the Board has issued under this Act (which has only been put in force when Asiatic cholera has been present in the kingdom) relate to the speedy interment of the dead; to house-to-house visitation; to the dispensing of medicines, guarding against the spread of the disease, and affording to persons afflicted by or threatened with such epidemic, endemic, or contagious disease, such medical aid and such accommodation as may be required.

5. *The Common Lodging Houses Acts.*—These Acts include the Common Lodging Houses Act, 1851 (14 and 15 Vict. c. 28), and the Common Lodging Houses Act, 1853 (16 and 17 Vict. c. 41). Under the Acts of 1851, the Sanitary Authority has power to make regulations for common lodging-houses, and the Medical Officer of Health is required by the Regulations of the Local Government Board (sec. 5) to advise his authority respecting these regulations. The suggested regulations issued by the Secretary of State, and which, if not in use now were in use to the time of formation of the Local Government Board, should be consulted. (See Glen's "Law Relating to Public Health," 6th ed. p. 263.)

6. *The Nuisance Removal Acts.*—These Acts include the Nuisance Removal Act for England, 1855 (18 and 19 Vict. c. 121); an Act to amend the Acts for the Removal of Nuisances and the Prevention of Diseases (23 and 24 Vict. c. 77); the Nuisance Removal Act for England (Amendment Act, 1863 (26 and 27 Vict. c. 117); the Nuisances Removal Act (No. 1), 1866 (29 and 30 Vict. c. 41); and the Sanitary Act, 1866 (29 and 30 Vict. c. 90). These Acts contain the greater number of the statutory provisions which affect the duties of a Medical Officer of Health.

The Nuisance Removal Act, 1855, gives a categorical definition of the word "nuisances." It provides (sec. 8) that this word shall include (a) any premises in such a state as to be a nuisance or injurious to health; (b) any pool, ditch, gutter, watercourse, privy, urinal, cesspool, drain, or ashpit, so foul as to be a nuisance or injurious to health; (c) any animal so kept as to be a nuisance or injurious to health; and (d) any accumulation or deposit which is a nuisance or injurious to health, provided that the accumulation be not necessary for effectually carrying on any

business or manufacture, and not have been kept longer than is necessary for the purpose of such business or manufacture.

The definition of the word "nuisances" here given was considerably extended by the Sanitary Act, 1866, as will presently be shown.

By sec. 11 of the Nuisance Removal Act, 1855, the Sanitary Authority is given power of entry to themselves or their officer, who for several purposes of the provision may be the Medical Officer of Health, (1) to ground proceedings in respect of nuisances; (2) to examine premises where nuisances exist; and (3) to remove or abate a nuisance in certain circumstances, and to inspect or examine any carcase, meat, poultry, game, flesh, fish, fruit, vegetables, corn, bread, or flour, under the powers and for the purposes of this Act.

With reference to sec. 13 of the Act, he may have to advise as to the sufficiency of privy accommodation, of means of drainage and ventilation, of the habitability and of the purification and disinfection of premises, and on the cleanly and wholesome keeping of animals, and of such other matters relating to nuisances which may be included in a justice's order for their abatement.

Under sec. 27 of this Act he may have to certify as to nuisances arising in cases of noxious trades, businesses, processes, or manufactures; and under sec. 29 he may be required to certify as to any house being so overcrowded as to be dangerous or prejudicial to the health of the inhabitants.

The Nuisances Removal Act, 1863, amends the Nuisances Removal Act for England, 1855, with respect to the seizure of diseased and unwholesome meat. This Act (sec. 2) gives to the Medical Officer of Health power at all reasonable times to inspect and examine any animal, carcase, meat, poultry, game, flesh, fish, fruit, vegetables, corn, bread, or flour exposed for sale, or deposited in any place for the purpose of sale or of preparation for sale, and intended for the food of man, and of seizing and examining the same in order to be dealt with by a justice, if the foods named shall appear to him to be diseased, or unsound or unwholesome, or unfit for the food of man.

The Sanitary Act, 1866, of the different Acts which have been referred to, contains the most numerous provisions affecting the

work of a Medical Officer of Health. In addition to the nuisances defined by the Nuisances Removal Act, 1855, this Act provides (sec. 19) that the word "nuisances" shall include (a) any house or part of a house so overcrowded as to be dangerous or prejudicial to the health of the inmates; (b) any factory, workshop, or work-place not already under the operation of any general Act for the regulation of factories or bakehouses, not kept in a cleanly state, or not ventilated in such a manner as to render harmless as far as practicable any gases, vapours, dust, or other impurities generated in the course of the work carried on therein, that are a nuisance or injurious or dangerous to health, or so overcrowded while work is carried on as to be dangerous or prejudicial to the health of those employed therein; and (c) any fireplace or furnace which does not as far as practicable consume the smoke arising from the combustible used in such fireplace or furnace, and is used within the district of a nuisance authority for working engines by steam, or in any mill, factory, dyehouse, brewery, bakehouse, or gaswork, or in any manufactory or trade process whatever; any chimney (not being the chimney of a private dwelling-house) sending forth black smoke in such quantity as to be a nuisance, with certain reservations set forth.

The particular duties which may be imposed upon the Medical Officer of Health under this Act may be enumerated as follows:—The inspection of his district from time to time with a view to ascertain what nuisances exist calling for abatement under the powers of the Nuisances Removal Act (sec. 20); the certifying as to cleansing and disinfecting of any house or part thereof, and of any articles therein likely to retain infection with a view to prevent or check infectious or contagious disease (sec. 22); advising as to necessary apparatus for disinfection (sec. 23); certifying as to removal to hospital of any person sick of any dangerous contagious or infectious disorder who is without proper lodging or accommodation, or lodged in a room occupied by more than one family, or being on board any ship or vessel (sec. 26); certifying as to the removal of any dead body of one who has died of any infectious disease retained in a room in which persons live or sleep, or of any dead body which is in a state such as to endanger the health of the inmates of a house

or room in which it is retained (sec. 27); advising as to rules for removing to hospital persons sick of dangerous infectious diseases brought into his district by any ship or boat (sec. 29); advising as to the regulations which a Sanitary Authority is empowered to make for houses or parts of houses let in lodgings or occupied by members of more than one family (sec. 35)—for a specimen of regulations adopted under this section, see Glen's "Law of Public Health," 6th ed., p. 273;—advising as to temporary places for the reception of the sick (sec. 37); advising and certifying as to the sufficiency of disinfection of houses in which infected persons have been lodging (sec. 39).

The Public Health Act, 1872, gives power (sec. 10) to the Local Government Board to define the duties of a Medical Officer of Health, any portion of whose salary is paid out of moneys voted by Parliament. It also authorises sanitary authorities (sec. 51) to direct the destruction of any bedding, clothing, or other articles which have been exposed to infection from any dangerous infectious disorders, and to give compensation for the same; and it will be the duty of a Medical Officer of Health to advise when such destruction may be necessary.

Under the provisions of this Act the Local Government Board issued Orders, on the 11th Nov. 1872, defining the duties of Medical Officers of Health in the urban and rural sanitary districts. The regulations contained in those Orders cover all the different duties which a Medical Officer of Health may be called upon to perform by his Sanitary Authority in the execution of the Sanitary Acts, and in addition they set forth those duties which he should perform in observing, recording, and advising on diseases within his district, and the action he should take on the outbreak of any contagious or infectious disease.

The foregoing review of the provisions of the Sanitary Acts, so far as they affect the action of the Medical Officers of Health, will within these limits define the meaning to be attached to the direction of the Local Government Board (sec. 4, Regulations) that the Medical Officer of Health "shall be prepared to advise the Sanitary Authority on all matters affecting the health of the district, and on all sanitary points involved in the action of the Sanitary Authority or Authorities; and in cases requiring

it he shall certify, for the guidance of the Sanitary Authority or of the justices, as to any matter in respect of which the certificate of a Medical Officer of Health or a medical practitioner is required as the basis or in aid of sanitary action." And also to the direction (sec. 5, Regulations) "that he shall advise the Sanitary Authority on any question relative to health involved in the passing and subsequent working of such bye-laws."

The subjects upon which he may be called to certify, as set forth in the statutes, relate to filthy and unwholesome condition of houses (P. H. A. 1848, sec. 60); overcrowding of houses (N. R. A. 1855, sec. 5); premises unfit for human habitation (Art. and L. D. Act, sec. 5); noxious trades (N. R. A. 1855, sec. 2); the necessity for cleansing and disinfecting houses (S. A. 1866, sec. 22); the necessity for the removal to hospital of persons sick of dangerous infectious disorder (S. A. 1866, sec. 26); the necessity for the removal of the bodies of persons dead of infectious disease, or of dead bodies in a state to endanger health (S. A. 1866, sec. 27); and the sufficiency of means taken for disinfection (S. A. 1866, sec. 39).

The bye-laws and regulations respecting which the Medical Officer of Health's advice may be required relate to the proper keeping of slaughter-houses (P. H. A. 1848, sec. 62; L. G. A. 1858, sec. 45); to the air-spaces about, and to the ventilation and structural arrangements as affecting health of buildings, and the closing of buildings unfit for human habitation (L. G. A. 1858, sec. 34); to the removal of refuse, cleansing of privies, &c., and keeping of animals (L. G. A. 1858, sec. 32); to common lodging-houses (C. L. A.); to sub-let houses (S. A. 1866, sec. 35); and to the removal from ship-board to hospital and keeping therein of persons sick of dangerous infectious disorder.

In respect to the duties which devolve upon a Medical Officer of Health, there are several important official papers, which it behoves him to consult. These papers are as follows:—

1. The suggested forms of Bye-laws issued by the Secretary of State in respect to the Local Government Act, 1858, with respect (*a*) to the prevention of nuisances arising from swine, filth, &c., and the keeping of

animals; (b) the removal of refuse and cleansing of privies, &c.; and (c) the regulation of slaughter-houses.

2. The Memoranda issued by the Medical Department of the Local Government Board, viz.:—

(a) General Memorandum on the Proceedings which are advisable in Places attacked or threatened by Epidemic Disease.

(b) Memorandum on Disinfection.

(c) Memorandum on Hospital Accommodation to be given by Local Authorities.

(d) Report on certain means of preventing Excrement Nuisances in Towns and Villages.

(e) Precautions against Cholera.

(f) Various Memoranda relating to Vaccination and Re-vaccination.

With regard to the incidence of the different duties above enumerated in urban and rural sanitary districts respectively, the Digest of Sanitary Statutes published under the authority of the Local Government Board should be consulted.

DIGEST OF THE SANITARY STATUTES.¹

THIS Digest, according to the prefatory note attached to it, has been prepared in the Department of the Local Government Board, by the direction of the President, the Right Hon. James Stansfeld, M.P., pursuant to a promise given by him to Parliament during the last session, when a question was raised with respect to the consolidation of the Sanitary Laws. The work is intended to furnish a clear and methodical exposition of the existing sanitary statutes. The language used by the compilers is "mainly that of the statutes themselves, stripped as far as

¹ "Digest of the Statutes relating to Urban Sanitary Authorities." (The Queen's Printers.) 1873.

"Digest of the Statutes relating to Rural Sanitary Authorities." (The Queen's Printers.) 1873.

practical of technical phraseology; and the several provisions relating to the same subject have been brought together under an arrangement of headings which will facilitate reference. No attempt has been made to construe the several enactments which appear to be in force, or to interpret any doubtful or contradictory passages contained in them. The work is intended as a digest only, and must not, therefore, in any way be regarded as an authoritative interpretation of the law." In an appendix to the Digest the Orders are given which have been issued by the Local Government Board, prescribing the mode of appointment and the duties of Medical Officers of Health and Inspectors of Nuisances under the Public Health Act, 1872.

This work must be added to the works of reference for the guidance of Medical Officers of Health recommended in the last number of the *Practitioner*. It will not supersede a work like that of Mr. Cunningham Glen's ("The Law relating to Public Health"), but it has a special value: first, from the digest of the statutes as they relate to urban and to rural sanitary authorities having been prepared separately, and that at a glance the reader is able to ascertain what comes under the purview of each authority; and next, from the classification adopted, the provisions relating to the same subject having been brought together. The convenience of this arrangement must be obvious, the number and complications of the Sanitary Laws and the considerable difference in the range of powers exercised by urban and rural sanitary authorities being regarded.

The laborious and difficult task involved in preparing this Digest has been thoroughly well done, and the redundancy of statutory phraseology most skilfully pruned. A good general index and excellent table of contents are attached to each division of the Digest. Not the least advantage of the work is its cheapness, the Digest relating to urban sanitary authorities being published at a selling price of eighteenpence; that relating to rural sanitary authorities at a selling price of ninepence. The typography, it may be added, is excellent.

THE CHOLERA.

FOR three successive summers there has been cause of alarm in this country lest Asiatic cholera, present on the Continent, should extend to our shores. It would appear, indeed, as if this disease had become naturalised in certain districts of Europe, and that in addition to the danger of extension of the malady to us from its original habitat in India, we have now also to regard the danger of extension from neighbouring countries where the malady has found a home, and whence it may spread independently of re-introduction from the remoter East. At any rate, it would seem certain that the standpoint from which in this country it has been customary to regard epidemic diffusions of Asiatic cholera must be changed, and that the conclusions of the International Sanitary Conference of 1866 must be reconsidered. The Conference held it to be demonstrated that "invading Asiatic cholera was never developed spontaneously, and had never been observed as an endemic (distinguishing secondary foci more or less tenacious) in any of the countries enumerated (namely, Europe, the Caucasian provinces, Turkey in Asia, the North of Africa, and two Americas), and that in these countries it had always come from without." In other words, in respect to Europe, as Grisolle had succinctly stated the question in 1857, "Asiatic cholera is endemic in India: it is only accidentally seen in Europe."

From the proceedings of the Conference it was left to be inferred, rather than categorically stated, that the great diffusion of cholera in Europe of 1852-55, like the diffusion of 1831-34 and 1847-49, were of Indian origin. The diffusion of 1852-55, and the remarkable diffusion in Western and Northern Europe of 1859 (not referred to by the Conference), had previously been most commonly held to be recrudescences of the diffusion of 1847-49. Data were submitted to the Conference which

¹ "The history of epidemic cholera in Europe, Northern Africa, and America, from 1847-48 to 1859, is that of a protracted invasion, with periods of recrudescence."—MR. NETTEN RADCLIFFE, *Eighth Report of Medical Officer of Privy Council*, p. 361.

showed at least that it was desirable to reconsider this question in respect to 1852-54. It was certain that of all the great diffusions of cholera in Europe, the history of this diffusion was least clearly known; and, as touching upon it, our knowledge of cholera in Persia and the Caucasus immediately preceding the diffusion was a blank. In 1871, Dr. Tholozan, the physician to the Shah of Persia, took up this question, and examined it by the aid of important additional data derived from Persian and Russian sources. The result of this examination was published in a masterly monograph, entitled "*Origine nouvelle du Choléra Asiatique, ou Début et Développement en Europe d'une grande Epidémie cholérique*" (Paris 1872). In this monograph Dr. Tholozan believed that he fully established three propositions, namely, (1) that the cholera of 1852-55 had its point of departure in Europe; (2) that it constituted a true epidemic; and (3) that it was linked to (in other words, developed out of the remnants of) the epidemic of 1847-50, which had never died out entirely in Germany and Bohemia. This view is in accordance with the opinions now known to be entertained by Dr. Pelikan, the Director of the Medical Department of the Russian Government.

Pursuing and extending this subject, Dr. Tholozan in a subsequent monograph entered into an examination of the question of duration of Asiatic cholera in Europe and America ("*Durée du Choléra Asiatique en Europe et en Amérique, ou Persistance des Causes productrices des Epidémies Cholériques hors l'Inde*," Paris, 1872), and from this examination he holds himself justified in formulating the following propositions:—"That Asiatic cholera, in Europe, as in India, can manifest itself in its three forms, endemic, epidemic, and sporadic. Its introduction into Europe," he continues, "has always given rise to violent and general epidemics when the period of immunity has been sufficiently prolonged. And after these epidemics have subsided, and apparently have become extinct, the disease may resume its activity and give rise to new regional or general epidemics which have all the original violent characteristics of the pestilence. In a word, Asiatic cholera, when introduced into Europe, manifests, so long as it persists, the same phenomena which distinguish the disease in India."

At the time when this was written the magnitude of the diffusion of cholera still in progress in Europe was only beginning to be seen, but its obvious important bearing upon the propositions just stated is briefly referred to in a postscript to the monograph cited.

This diffusion began at Kiev in the autumn of 1869, and in the following year, 1870, cholera spread extensively in the provinces of Southern and Central Russia, and in Transcaucasia, appearing also in Moscow and in St. Petersburg. In 1871 the disease was general throughout Russia in Europe, and it was scattered beyond the Russian frontier, in parts of East and West Prussia, in Sweden, at Hernosand, and in Turkey on the south bank of the Danube, and at Constantinople. In 1872 cholera persisted in many parts of Russia, particularly in the valleys of the Dnieper and the Bug, and it passed across the Austrian frontier into Galicia, and subsequently extended widely in Hungary, and showed itself in many places in Moravia and in several districts of Bohemia. Throughout the winter of 1872-73, cholera was more or less prevalent in the northern provinces of the Austrian Empire and in Hungary. With the advance of the spring of the present year and in the early summer the disease again became active. It was carried by raftsmen down the Vistula to Dantzic. It spread along the southern bank of the Danube to its mouth, and extended inland in Bulgaria; it crossed the Austro-Italian frontier and entered the province of Venetia, and appeared in Venice itself. Finally, it has shown itself and appears to be now spreading in Venice.

Of this diffusion, as of the diffusion of 1852-55, Dr. Pelikan affirms that it is of European origin; and that it was developed at Kiev out of the embers of the epidemic of 1865-67, which had never entirely subsided in Russia. In this view Dr. Tholozan concurs.

The importance of the opinion here expressed, in view of the protection of this country and of Central and Western Europe against cholera, cannot well be exaggerated. It should need no argument to show that, if we are to conclude that epidemics of Asiatic cholera may now take their origin in Europe as well as in India, our measures of precaution against this disease may in

several respects have to be very greatly modified. In fact, cholera, in place of being regarded and dealt with as an occasional contagion, will have to be looked upon and treated as a "current contagion" (to use Mr. John Simon's phraseology). The measures of precaution against it must indeed be approximated to those which are in force against common contagious diseases.

But before the conclusion as to the origin in Russia of the recent diffusion of cholera in Europe is accepted, it is much to be desired that the detailed data upon which it is founded should be forthcoming. As the question stands, the conclusion is open to considerable doubt. This has been shown in a report prepared by Mr. Netten Radcliffe for the Medical Officer of the Local Government Board, and laid before Parliament last session. In this report Mr. Radcliffe showed that information was wanting to prove the filiation of the outbreak of 1869 at Kiev with the epidemic of 1865-67; that the outbreak had been preceded by great activity of cholera in Persia in 1868, and this activity had been preceded by cholera migrating into Afghanistan from North-western India in 1867. Mr. Radcliffe also showed that the real nature of the outbreak at Kiev had not been recognised by Russian physicians until some time after the occurrence of the earlier cases; and that there was no evidence either of knowledge of or search after possible connection with probable migrating cholera from India through Persia. He showed further, also, that there was a possible unsuspected route through which cholera might have passed from Persia into South Russia.¹ Mr. Radcliffe frankly avowed that his doubts might arise solely from the want here of information, in the possession of the Russian Medical Department, but not made public. As an independent consequence of Mr. Radcliffe's examination of the question, he pointed out that from the rapid growth of railway communication between Central Russia and Central Europe and South Russia, and of steam communication between the South Russian ports and the Transcaucasian port of Poti, and of railway communication between this latter port

¹ As this article passed through the press our attention was directed to an article by Mr. C. Macnamara in the *Indian Medical Gazette* for September 1, 1871, in which Mr. Radcliffe's doubts were substantially anticipated.

and the shore of the Caspian, Persia would quickly be brought into such close connection with Europe, that contagions current in that country would have to be held for all practical purposes to be current in Europe also.

Mr. Radcliffe's report has been transmitted, at the suggestion of Mr. Simon, to foreign Governments, and no doubt the questions which he has raised will receive the attention of the Russian Government, and it is to be trusted will elicit a reply from its Medical Department, with the data upon which the opinion of Dr. Pelikan has been based.

Meanwhile, assuming provisionally that the existing diffusion of cholera is of Russian origin, and that it is possible for epidemic extensions of the disease to become developed in Europe, it must be obvious that so long as Asiatic cholera is present in any part of the Continent, so long this country is exposed to the danger of an invasion from the disease at any time becoming actively diffusive.

The Local Government Board has issued an Order (dated 17th July, 1873), directed to Sanitary Authorities, Officers of Customs, and masters of ships, requiring the inspection of ships infected with cholera or coming from ports where cholera exists, and setting forth the conditions under which such inspection is to be performed. This Order amends in several respects the Orders of Council as to cholera of the 9th July and of the 3rd and 5th August, 1871, for which the present Order is substituted.

The Local Government Board has also caused to be issued a memorandum of "Precautions against Cholera," prepared by its Medical Officer, Mr. John Simon.

THE RELATION BETWEEN ENTERIC AND SCARLET FEVERS.

SOUND hygienic practice depends so much on accurate notions of the etiology of infectious diseases, and this in its turn depends so essentially on a sound pathology, that any contribution to the pathology of these epidemic diseases cannot fail to have the greatest interest for the student of hygiene. Accordingly we have turned with some eagerness to a paper published in the last volume of the "Medico-Chirurgical Transactions" (vol. xxxvii. 2nd series), by Dr. John Harley, on "The Pathology of Scarlatina and the relations between Enteric Fever and Scarlet Fever." In this paper Dr. Harley again raises a question which he broached a few years ago, and from the analysis of the morbid appearances found in twenty-eight cases of scarlet fever he comes to the conclusion that "the pathology of scarlatina is precisely that of the first stage of enteric fever," and he proposes to term the latter disease "*Abdominal Scarlatina*" (p. 138).

Dr. Harley's paper is an excellent illustration of the confusion that exists in the minds of some sensible physicians between pathology and morbid anatomy. To anyone who has experience of the pathology, in the true sense, of scarlatina and enteric fever, what can be more startling than the following passage?—

"If we now take the pathological conditions into one general view, it will appear that *febris lymphatica* is the appropriate scientific definition of scarlatina; that death is very likely to occur during the first week of the disease from the formation of fibrinous clots in the heart and great vessels; that the condition of the biliary function is such as to lead to an outbreak of diarrhoea; that mesenteritis and enteritis, sometimes general, but usually confined to the solitary and agminated glands, exist from the third day and onwards during an attack of scarlet fever, both being at their acme during the height of the fever, i.e. from the third to the seventh day; that the enteritis is usually latent, but ready to declare its presence upon slight provocation; and that this inflammatory condition of the mesen-

teric and intestinal glands may persist to the sixty-ninth day. From this view *one general conclusion as to the connection of scarlet fever and enteric fever is inevitable, viz. that the pathological changes accompanying an attack of scarlatina include all those of the first stage of enteric fever, and are so far identical with them.* And it follows, therefore, that the transition from the former to the latter is nothing more than a natural pathological sequence, readily determined by any cause which may increase the intestinal irritation."

Dr. Harley had evidently been thinking exclusively of the anatomy of disease when he wrote this passage. Even in respect of anatomy, when one comes to analyse his cases, it is seen that the fact of *something being wrong with the same part* of the body is regarded by Dr. Harley as evidence of pathological unity. Yet, what would he himself say if one confounded, for instance, tuberculous ulcers of Peyer's glands with the ulcers of enteric fever; or if, what is scarcely less extraordinary than the above statement, one should say that a disease affecting anyhow the breast and the lymphatics connected therewith was, of necessity, some stage of cancer? When, however, it is considered that the identity of two things that present certain anatomical similarities wants to be considered by the light of etiology and of phenomena during life, there is still more curious confusion in Dr. Harley's argument. There is throughout the paper no evidence that the contagion of scarlet fever can produce enteric fever, or *vice versa*, and apparently no thought that such evidence is of moment. There is no consideration as to incubation time of the two diseases; and there is no recognition of the huge points of dissimilarity between the clinical phenomena of the two. In fact, the only reason adduced for the identity of the two things, apart from some slight similarity in the post-mortem appearances of certain cases of scarlet fever to those of certain cases of enteric fever, rests on the circumstance that sometimes enteric fever and scarlet fever occur in the same person, the one disease either soon before or at the same time as the other. And why should they not? The notion of "mutual exclusion" of epidemic diseases must be given up by anyone who examines the statistics of the two diseases from the obvious arithmetical standpoint. At any rate, no such notion

is present to Dr. Harley. And in the absence of any such "mutual exclusion," it must occasionally happen that the same person will be the subject of two diseases at the same time, or within the same short period. Suppose a number of susceptible persons to be exposed, in the same twelvemonth, once to the contagion of scarlet fever, and once to the contagion of enteric fever; the chances are against the two things occurring together. But there will of necessity be a certain minority of cases where the phenomena of the two diseases will concur. To say that this concurrence means that the two diseases are identical is surely untenable. Not scarlet fever and enteric fever only, but enteric fever and small-pox, typhus and scarlet fever, small-pox and enteric fever, and any other combination that can be proposed, must be expected occasionally to occur at the same time in the same person. We know they do so. But on that account, we do not affirm that enteric fever is small-pox, or that typhus is scarlet fever, or that small-pox is enteric fever. Dr. Harley gives us no help to reckon what was the simple likelihood of concurrence in each of his cases. They are for the most part cases of *one disease following on another*, and they are derived from fever-hospital practice; where the opportunity of a second disease being contracted has actually been present during the sojourn of a patient admitted for a first disease. Or else they are cases where, as Dr. Harley himself observes, the poison of scarlet fever was affecting the whole family of the sufferer. Why should the fact of the patient having been thus exposed to the poison of scarlet fever keep him safe from the poison of enteric fever if he comes into relation with it? or, conversely, why should he be proof against the scarlet fever poison because he chances to have got the poison of enteric fever? There would appear to be no reason; and one's judgment on this part of Dr. Harley's argument must of necessity be at least suspended until one knows what have been the chances of accidental concurrence. Towards this, as we have said, Dr. Harley gives us no help. For all that can be seen to the contrary, there were special causes of enteric fever operating on his scarlet fever convalescents, and others besides them may have been affected at the same time by these causes. Indeed, if we are not mistaken, something of this kind did actually

occur some few years ago in the hospital from which Dr. Harley gets his experience. We believe that enteric fever, propagated by a faulty and infected local sewer, did affect the scarlet fever patients, but that it also affected other people; and, moreover, that on the removal of the sewer cause, the cessation of the enteric fever (among scarlet fever patients as well as among others) instantly followed.

From his own point of view, in fine, it is clear that Dr. Harley has not done justice to his thesis, when he omits to consider whether scarlet fever and enteric fever have any difference in incubation times; whether the throat symptoms of scarlet fever have or have not anything special in them; and whether the definite tendency to albuminuria on the eighteenth day in scarlet fever can fairly be compared with the occasional and irregular albuminuria of certain cases of enteric fever. The essential defect of his paper consists in his having regarded every anatomical similarity, however small, as indicating unity, while he has ignored far broader differences, anatomical, etiological, and symptomatic; treating these, apparently, as matters not worthy of consideration.

THE PRACTITIONER.

SEPTEMBER, 1873.

Original Communications.

NOTES ON GUARANA.

BY W. MACDOWALL, M.D. (EDIN.),

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MY only reason for placing the following notes on record is, that they may assist to establish the reputation of guarana as a remedy for sick-headache. Those who, up to the present time, have used the drug for this troublesome disease, have not been by any means unanimous in their opinion regarding its efficacy. Although the number of cases of sick-headache which I have treated is comparatively small, the results have been so uniformly successful, that to my mind the drug has a claim to much more extensive use.

Having become convinced of the decided influence of guarana in the cases for which it is usually prescribed, I tried it experimentally in a few cases of epileptic excitement. Work on other subjects has prevented me continuing these investigations far enough to obtain anything like conclusive results. It is therefore my intention to continue this class of observations, and, if the results should prove of sufficient value, to publish them. In the meantime I shall content myself with giving a few of

the cases of epileptic excitement in which guarana was tried, and the effects, &c., carefully noted.

In addition to these cases, it may be not uninteresting to give the details of some observations which I made on myself and others as to the influence of guarana upon the temperature, circulation, and digestion in a state of health.

The cases of sick-headache treated with guarana are as follows:—

CASE I.—M. N——, aged 34. This lady has suffered from sick-headache for a great number of years, indeed from childhood. Her attacks were very severe until about eight years ago, when she removed from an inland country town to a residence near the sea. She states that until she was about twenty years of age she was in somewhat delicate health; not suffering from any disease in particular, so far as she knows, excepting her headaches, but was languid, weakly, and unfit for active exertion or occupation. After that date her health improved, and has continued excellent up to the present time. She attributes this establishment of her strength to a prolonged residence in the country.

Since early childhood she has suffered from sick-headache, and her tortures have continued with increased severity since she fell downstairs and injured her head. This accident occurred when she was about nine years of age. She was unconscious for some hours after the fall, and was confined to bed for some days.

The following is her account of the sick-headaches as they occurred before they decreased in severity eight years ago:— They occurred, as a rule, every three weeks; and she never escaped for longer than a month. She experienced no previous derangement of digestion; but when an attack was imminent, she frequently had uneasy sensations at the pit of the stomach. These would gradually increase in intensity during a few hours, and generally ended in vomiting. As a rule, however, these preliminary stomachic symptoms were absent. Her first head-symptom was a slight throbbing pain, passing, as it were, between the temples or between the forehead and occiput. At the same time, a feeling of great depression, with sickness, always appeared. The characteristic symptoms then gradually

increased in intensity, and in from three to six hours the attack usually reached its height. Before this stage, however, had been attained, she became quite prostrate, and was obliged to go to bed. These attacks were always, sooner or later, accompanied by vomiting; the act of emptying the stomach increased the pain in the head to an almost unendurable degree; but when it was completely emptied of its contents—usually highly bilious matter—some relief to all the symptoms followed. On an average, the headache continued at its maximum during six or seven hours; an hour or two after vomiting, the patient generally fell asleep, and then awoke in the morning free from all her previous miseries, but feeling extremely weak and languid.

Shortly, it may be stated that the attacks of sick-headache during the past eight years have been, in their leading features, similar to those already described, but somewhat less intense.

The following have been the results of treatment with guarana:—

February 1. — Slight symptoms appeared shortly after rising in the morning, and continued to get worse until 7 P.M. At this time she presented all the indications of an intense attack, and retired to her room quite unable to bear up longer against her suffering. She now took 3ss of guarana, with the, to her, very unexpected and satisfactory result, that in about twenty minutes all sickness and headache had disappeared; and she felt, as she expressed it, like another woman. The drug appeared to her to act like a charm; and so much pleased was she with the result, that she presented herself for inspection to some who had but shortly before witnessed her distressed condition. The improvement in her state was most marked and surprising, for, from being almost quite prostrate, she was at once able to resume her somewhat arduous duties. She also escaped without the occurrence of vomiting.

February 22.—During the morning, the usual premonitory symptoms appeared. At once she took 3ss guarana, the result being that, instead of the attack running through all its stages, it disappeared entirely in less than half an hour.

April 8.—She awoke in a state of considerable depression; during the day a typical sick-headache became developed; but

from circumstances she did not take any medicine until 5 P.M., at which time the symptoms were very severe. No benefit resulted from the half-drachm of guarana which she then took. At 9 P.M., still feeling extremely ill, she took another 3ss, and in an hour all the symptoms had disappeared. During the whole day she had been unable to take any food, yet at 10 P.M. all nausea had so entirely disappeared, that she was able to take a hearty supper.

Up to the present time (May 8) she has had no return of sick-headache.

CASE II.—R. D——, aged 30 ; nurse. She states that she has suffered from sick-headache from childhood. The attacks, however, have not been quite so severe during the past four years. This change in their character she associates with her change of residence, for four years ago she left her native county, Leicester, and has since that date resided in Yorkshire. During childhood the attacks were not frequent—perhaps every two or three months; but after reaching puberty, they occurred regularly every month for a number of years. They invariably preceded the appearance of the menstrual flow. As she advanced in age, the attacks occurred at other times besides the menstrual one, so that on an average they came on once a fortnight. None of her brothers or sisters were affected like herself, but her mother suffered severely from sick-headache as long as she lived.

The usual course of attack is as follows :—After slight headache has existed about half an hour, she generally becomes slightly sick. The pain is always confined to the forehead, and is described by her as being of a burning, throbbing character. As the attack proceeds, the pain in the head becomes almost intolerable, and she is in the habit of going to bed, that she may obtain some slight relief through rest and quiet. The sickness rarely terminates in vomiting, but there is intense loathing of food, her appetite having entirely disappeared. On rare occasions the headache has remained at its maximum of severity for from twelve to twenty-four hours; but the average duration of this period may be stated at about six or eight hours. When an attack is disappearing, the sickness always disappears first; the pain in the head then lessens, becoming of a dull and

depressing character; then, in about two hours, all disagreeable symptoms disappear, and the patient regains her usual health, being quite free from any depression or exhaustion.

Since her residence in Yorkshire, her headaches have not, as a rule, been so frequent, occurring not oftener than every six weeks; and it has even happened that she has been free from them for three months.

On four occasions she has been treated with guarana, and always with the happiest results.

March 1.—She awoke in the morning suffering from severe sick-headache, although at that time it had not obtained the usual maximum of intensity. She took half a drachm of guarana at once, and experienced decided relief in about half an hour. During the remainder of the day she suffered from a dull uneasiness in the head, but was quite able to go about her usual duties.

March 31.—After travelling, the premonitory symptoms of an attack appeared. On reaching home she at once took half a drachm of guarana, and in less than an hour was quite free from all inconvenience.

April 27.—Awoke with headache, but, being inclined to try if it would pass off without treatment, she delayed taking her usual dose of guarana. Feeling no better, however, she lay down in bed about 2 P.M. for a short time, but without relief. She then took half a drachm, relieving the sickness immediately and the headache very soon afterwards.

May 10.—About midnight she began to suffer severely from sick-headache, and continued ill all night. In the morning she took a little tea, which made her sick, but with no relief to headache. At 8 A.M. she took the usual dose of guarana, but with no result; at 10.30 A.M. it was repeated with like result; at 2 P.M. another half-drachm was taken, which gave relief very soon afterwards.

CASE III.—M. M.—, aged 25, single, has suffered from sick-headaches from childhood; indeed, she does not remember a period of her life when she did not suffer from them. When she was a child, they occurred about once a fortnight; but when she reached puberty, they became less frequent, the intervals increasing to four or five weeks. There has never been any

connection between the occurrence of menstruation and the date of appearance of a sick-headache. For a number of years this young woman has been in delicate health. She labours under phthisis of a very chronic description, though the disease has occasionally assumed a more active form for brief periods, and there have been several rather smart attacks of hæmoptysis. Of late, however, with proper attention to her health, she has continued in moderately fair general condition, and quite able for her work as a nurse, but at the same time very much troubled with headaches.

Since her general health became delicate, her attacks of sick-headache have been much more frequent, and indiscretion in diet brings one on at once. As a rule, they begin during the night. The first symptom is a dull, throbbing pain in the temples; it gradually increases; then sickness comes on; so that in a few hours she is frequently necessitated to lie down in bed. Should the attack prove a mild one, it may pass off in six or seven hours; but it is more common for it to continue during a whole day. On rare occasions, when particularly severe, it has continued for two days. After a sick-headache has continued at its maximum for about an hour, the patient invariably vomits a small quantity of bilious fluid, sometimes with some slight relief to the pain in the head.

April 16.—Patient awoke in the morning with sick-headache, which continued intense during the day, until 6.30 P.M., when she took half a drachm of guarana; and in half an hour she felt quite relieved.

May 10.—Awoke this morning: sick-headache in first stage. Shortly after taking the usual dose of guarana, the symptoms began to abate; and by 9 A.M. she was quite free from all discomfort.

CASE IV.—H. A——, aged 23. She enjoyed fair health in every respect until three years ago, when she had a slight attack of small-pox. Shortly afterwards sick-headaches appeared for the first time in her life, and have continued to recur at intervals ever since. They came on without any special known cause, and are separated by intervals of a fortnight on an average. They were not at first very severe, but now they are very marked, and occasion much distress and inconvenience.

Her attacks present no peculiar feature. They invariably begin soon after breakfast by the occurrence of a dull, throbbing pain in the forehead. As the pain gradually increases, she becomes sick during the afternoon, vomiting usually highly bilious matter. The headache continues during the whole of the following night, but generally allows her to have snatches of sleep. During the next day it usually disappears, though occasionally it has happened that it has continued into the third day. During its continuance she loathes all food, though the tongue continues clean, and bowels regular. There is no connection, as to time of occurrence, between menstruation and sick-headache in her case.

April 5.—After having suffered from a well-marked sick-headache for thirty-six hours, she got half a drachm of guarana from one of her neighbours, who had already experienced benefit from its use on several occasions. She stated that in half an hour at most it had entirely disappeared.

May 1.—She again tried guarana with complete success. By taking the powder almost immediately upon the appearance of the attack, it was cut short, all uneasiness disappearing in a short time.

CASE V.—*M. S*——, aged 26 ; married, but no family. She has suffered from sick-headache since childhood, but in all other respects she has had good bodily health. Slight attacks are of frequent occurrence, but they only continue a short time, and pass away if the patient lies down and takes some strong tea. The more severe attacks, however, generally continue for the greater portion of a day, and quite incapacitate her for her duties. The pain usually appears suddenly in the forehead, and is at first of a dull, heavy character ; but in a short time it throbs violently, and is accompanied by an intense feeling of sickness.

March 20.—A headache of the usual character appeared about noon ; she immediately took half a drachm of guarana. She states that in half an hour at most she was quite free from pain and sickness.

Since that date she has suffered five times from sick-headache ; each time she has tried guarana, and always with success. No inconvenience, such as constipation, followed the administration of the drug, except on one occasion, when she felt somewhat

faint ; but she could not attribute this to the action of the drug, because she has often experienced similar sensations before.

It may be added to the history of this case that the headaches are generally very severe on the day preceding, and on the day of appearance of menstruation. In her case, also, the affection appears hereditary. Her mother suffered severely from sick-headache.

CASE VI.—E. P——, aged 66, was admitted into this asylum on February 14th, 1854. She then suffered from mania, but gradually passed into a state of dementia, in which she continued for many years. About 1866, she exhibited some signs of improvement, which have continued so satisfactorily, that she now converses rationally and cheerfully, and is very industrious ; her case is now under consideration for discharge.

On two occasions she has tried the virtues of guarana ; but in her case the results have not been quite so marked as in those already given. There was some mitigation and shortening of the attack ; but the patient rather objected to further treatment, for the somewhat peculiar reason that she looked upon such afflictions as sent directly by Divine power, by which alone they could be effectually relieved. She has suffered from sick-headache during the greater part of her life, and appears quite resigned to the prospect of continued suffering during the remainder of her pilgrimage here.

In passing now to the consideration of those cases of epileptic and maniacal excitement in which guarana was used, it is necessary to make one or two preliminary remarks. The very worst patients were selected ; those who were notorious for their violence and noise. Such being the character of the cases, it is not to be wondered that thermometric and other modes of minute clinical observation had to be dispensed with. It is very evident that the clinical value of the record would have been much increased had it contained many facts which, however, it was absolutely impossible to obtain.

CASE VII.—L. C——, aged 19 ; admitted August 4, 1871. When admitted, he was in a state of wild excitement, raving constantly on religious matters. He stated that he had been commissioned by God to preach the Gospel, and that he had seen two angels, &c. His mother informed the medical officers

that when he was about three years of age he had sun-stroke, and since that time had suffered from fits, the attacks occurring at intervals of from one to three weeks. No marked mental effect followed these seizures until a week before admission, when he became peculiar in conduct, and at last quite maniacal and unmanageable.

It is quite unnecessary to reproduce the very full notes which have been made of his condition during his residence here. He was, however, almost constantly more or less excited until the end of 1871, in spite of various treatment. It was also discovered that he was a furious masturbator. He continued very quiet, though somewhat demented, from January until April 1872, when he became exceedingly excited and obscene. Between that date and the time he was tried with guarana he had several severe attacks of excitement, accompanied with great violence. It was found that his condition could be greatly improved by ergot. Whenever, therefore, an attack of excitement appeared, he was at once ordered liq. ext. ergot ʒij. three times a day. A peculiar feature in his case is, that attacks of excitement are always preceded by marked flushing of the face; and it continues until the excitement disappears.

February 28.—To-day he became excited, with very marked flushing of the head and neck. After the administration of a drachm of guarana, he was decidedly quiet for two hours; he then gradually relapsed into his previous condition. During this temporary calm he appeared pale, and as if depressed; indeed, his condition suggested to the attendant that the patient was about to faint or be sick.

March 7.—Patient again excited. A drachm of guarana reduced his violence, and caused blanching of the face for fully two hours. After this time all his former symptoms returned, so that in another hour he was as bad as ever.

March 16.—Guarana treatment again tried, and with results like those already mentioned.

March 22.—Patient again very much excited; treatment repeated with a fairly good result, so long as it continued. The excitement and violence, however, returned in less than two hours, and were as severe as before.

March 25.—He was to-day again tried with a drachm

of guarana, with the usual temporary, but no permanent benefit.

April 4.—Being again violent and troublesome, he took a drachm of guarana with decided effect. He was very quiet for fully two hours. As usual, there was marked pallor of the face.

It must be admitted that in this case the inferiority of guarana to liq. ext. ergot. was evident enough. It was demonstrated, however, that the former had a marked temporary effect in an exceedingly bad case of epileptic excitement, though it did not appear to have any effect upon the frequency or severity of the fits, and did not derange digestion in any way.

CASE VIII.—C. P——, aged 38. Although the preceding case did not give much encouragement to continue experiments in the use of guarana in epilepsy, yet, simply for opportunity of observation, it was given to this man—one of the worst epileptics and most dangerous men in the asylum.

Fully two years ago he was picked up by the police and placed in a workhouse, where he resided about four months. At that time he had one or two fits every fortnight, but at last, becoming unmanageable, he was transferred here. When admitted, he was in his placid state, a condition of well-marked dementia; but in two days he became highly excited and violent, assaulted everyone near him, and declared himself to be the master of the whole establishment. Since that date he has proved himself a most dangerous fellow, and has been engaged in desperate conflicts with neighbours and attendants. It was found, however, that these attacks could be greatly lessened in severity and duration by allowing him to rest in bed, and by giving him, for continued periods, liq. ext. ergot. Well-marked flushing of the head and neck always appears previous to and during his excitement. The only effect produced upon him by guarana was slightly to lessen that vascular activity. His excitement, in spite of rather large doses, remained quite unabated; so that in the long run recourse had to be made to liq. ext. ergot., and that with the best effect.

In order that the guarana should not get any credit due to the action of ergot, the latter drug was stopped for some time before the other was tried. As was to be expected, an attack of

the usual character was not long in supervening, and guarana was tried as follows:—

March 18.—Patient much excited: half-drachm of guarana given at 8 P.M. No result.

March 19.—Half a drachm given at 7 A.M., and another at 9 A.M.; but with no result, except that the patient appeared paler.

March 24.—For the last few days he has been moderately quiet, if quite undisturbed; but to-night, after supper, he became much worse. A drachm of guarana given, with result as before.

March 25.—Still desperately excited, and violent if approached. During the course of the day he had a drachm and a half of guarana, but with no result. It did not appear to constipate him.

April 5.—Again excited; a drachm of guarana: no result.

April 6.—Two drachms were given this morning, with an interval of two hours: no result.

April 20.—He is again excited: ordered one drachm of guarana at once. It had no effect, and it was found impossible to persuade him to take more medicine.

May 5.—He became excited during the afternoon: was ordered a drachm of guarana at bed-time.

May 6.—Being very much excited, he was ordered one drachm at 6.30 A.M., and another at 9 A.M.

As it was evident that no benefit resulted from this form of treatment, it was abandoned in favour of liq. ext. ergot., which had always been found of such signal service in his case.

I have used the drug in other cases of epileptic excitement, and in two or three cases of mania; but I do not consider the observations in these instances extensive enough, or made with sufficient accuracy of detail, to warrant me reproducing them. I shall therefore pass to my third class of observations on the action of guarana. These were made upon myself and two male attendants. My object was to discover what physiological action, if any, it possesses. It soon became evident, however, that even in very large doses its effect upon the body in a state of health is almost, if not quite, inappreciable. Its power to cause constipation is very feeble indeed, as is proved by the fact

that, after taking an ounce of guarana daily for nearly a week, the bowels continued to act normally. In large doses, it does appear to dispel drowsiness, and is so far an aid to sustained work, by helping a man to begin and continue his work even after a late dinner. What its action on the chemical constitution of the urine may be, I cannot say. In my own case it did not at all influence the occurrence of several slight attacks of phosphatic gravel, to which I am rather subject when more than usually hard-worked.

The following are the details of the experiments on the attendants and myself:—

J. D. G——, aged 28, unmarried, was a soldier, and had been in India, but is in excellent bodily health.

March 10.—Took half a drachm of guarana at 10 P.M.: no effect.

March 11.—Took a drachm at 10 A.M.: had persistent twitching of the eyelids. Another drachm taken at 10 P.M. He awoke at midnight, and complained of a tightness across the forehead. He fell asleep shortly, and had no return of pain in the head when he awoke next morning.

March 12.—Took a drachm at 10 A.M., another at 3 P.M., and a third at 9 P.M. No result followed, except very slight constipation.

March 13.—Took three drachms during the day, but with no result.

March 14.—Powders repeated as before, and with a similar effect.

On March 15, and the day following, he took three drachms each day; on the 17th he took four drachms. He might apparently have continued swallowing guarana at the same rate for an unlimited period, but he began to suffer from influenza. The attack was very severe, necessitating his confinement to bed. Some of his neighbours suggested the probability that the influenza was the result of the powders which he had been taking. Accordingly, when he got better of his attack of cold, he exhibited such a disinclination to be again sacrificed for the benefit of science, that I did not attempt to argue with him on the matter.

He bore decided testimony as to the power of guarana pre-

venting or removing drowsiness after food. Having been a very abstemious man all his life, he found, on coming here as an attendant, that the very liberal diet, consisting largely of animal food, had a very soporific effect upon him. Indeed, if I mistake not, he got into trouble on one occasion for falling asleep after dinner when he was on duty. He found that a drachm of guarana entirely prevented this heaviness, and he did not experience that feeling of congestion of the head which occurs so often after a liberal meal.

J. G.—, the brother of the previous case, took an ounce of guarana in drachm doses. He, however, became suspicious, when he observed his brother suffer so severely from influenza, that it was not altogether so harmless a drug as it had been represented to him. He therefore gave it up. It produced no effects upon him, except slight constipation; and he thought he was more lively and readier for work than usual.

Concerning the observations made upon myself, I may say that they were conducted with great care, and were as follows:—
1. I took the drug for some time, but experiencing no effect, gave it up, that I might begin some observations as to its effect upon the temperature of the body. 2. By a series of observations I ascertained my average temperature for all periods of the day. 3. Having done so, I resumed the use of guarana, and continued the thermometric observations at the same time.

The following are the details of the first series of experiments:—

March 3.—At 9.30 P.M. took 30 grs., with no effect; took 30 grs. at 11.30 P.M., and went to bed to read: still not the slightest effect.

March 4.—Took 30 grs. at 9.30 A.M.: no result beyond slight pulsation in head, which was probably not due to the drug. Took 45 grs. at 2.45 P.M.: no result; 60 grs. at 8 P.M.: no result, except that I appeared in unusual humour for continued hard work.

March 5.—11 A.M., 90 grs.: the same at 10.30 P.M. At 11.45 P.M., bowels operated for the first time for about seventy-two hours; a very unusual occurrence for me to be so long constipated.

March 6.—Took four doses of guarana, each containing 90 grs., during the day: no effect.

Omitted drug from 7th to 9th March.

March 10.—At 10.30 A.M., took a drachm; repeated the dose at 4.30, and again at 10.45 P.M. From 9 P.M. until bedtime my head felt rather confused; I was quite "out of sorts," and unable to settle to work.

March 11.—A drachm at 10.30 A.M. Bowels operated at noon; decidedly constipated. At 12.30, took a drachm in beer. During the remainder of the day I felt unsettled, restless, and unable to work with vigour. There was a really distressing fullness and throbbing in the head, chiefly in the temporal regions.

March 12.—Took a drachm at 9.30 A.M., another at noon, and a third at 3.50 P.M.: all with no effect.

March 13.—Took a drachm at 9.30 A.M., another at 1 P.M., another at 8 P.M., and a fourth at 11 P.M. Bowels moved normally at 10.30 A.M., and a small motion occurred at 8 P.M.

March 14.—To-day the dose was doubled: two drachms at 9.30 A.M., two at noon, two at 3.30 P.M., and two at 11.30 P.M. Bowels operated normally twice during the day; in the morning at 11.45, and in the evening 10.15.

Being satisfied that but little could result from a continuation of such experiments, I stopped taking the drug, waited for nearly a week, and then began some observations on my normal temperature and pulse-rate. During this time I continued my ordinary duties and diet. For some minutes, on an average for a quarter of an hour, I rested on a couch before taking the rate of the pulse. As nearly as possible, the observations were made at the same hour on each day.

March 21.—9 A.M.: P. 80, T. 98°·6. 12.45 P.M.: P. 78, T. 98°. 4.30 P.M.: P. 73, T. 98°. 9 P.M.: P. 74, T. 97°·8. Midnight: P. 76, T. 98°·2.

March 22.—At 8 A.M.: P. 66, T. 98°·4. 1 P.M.: P. 79, T. 98°·4. 7 P.M.: P. 87, T. 98°·7. 11.35 P.M.: P. 74, T. 98°·2.

March 23.—At 8.30 A.M.: P. 68, T. 98°·4. 12.30 P.M.: P. 74, T. 97°·8. 4 P.M.: P. 78, T. 98°. 10 P.M.: P. 74, T. 98°.

March 24.—At 8 A.M.: P. 70, T. 98°·2. 12.15 P.M.: P. 71, T. 98°. 3.15 P.M.: P. 82, T. 98°·4. 7.15 P.M.: P. 88, T. 98°·2.

March 25.—12.30 A.M.: P. 80, T. 98°. 8.30 A.M.: P. 66., T. 98°. 3 P.M.: P. 73, T. 98°·2. 10.30 P.M.: P. 76, T. 98°.

March 26.—8.30 A.M.: P. 72, T. 98°2. 3 P.M.: P. 72, T. 98°. 11 P.M.: P. 76, T. 97°8.

March 27.—12.15 A.M.: P. 72, T. 98°2. 8.15 A.M.: P. 70, T. 98°4. 3 P.M.: P. 72, T. 98°2. 10 P.M.: P. 72, T. 98°2.

An average of these figures gives the following:—

During the morning the pulse was 70, the temperature 98°3; at midday, P. 75, T. 98°; during the afternoon, P. 75, T. 98°1; at 9 P.M., P. 78, T. 98°1; and at midnight, P. 75, T. 98°.

The following are the details of the observations made whilst I took guarana in large doses:—

March 28.—At 2.30 P.M., P. 72, T. 98°6. At that time I took a drachm, and at 4.15 P.M. found no change—P. 72, T. 98°6.

March 29.—At 7.45 A.M.: P. 72, T. 98°2; took one drachm. At 8.20 A.M.: P. 66, T. 98°4. At 3.45 P.M.: P. 81, T. 98°2; took a drachm. At 5 P.M.: P. 74, T. 98°2. At 8 P.M.: P. 76, T. 98°; took another drachm. At 10.30 P.M.: P. 74, T. 98°.

March 30.—At 8.30 A.M.: P. 72, T. 98°2; took a drachm. At 11 A.M.: P. 76, T. 98°. At 2 P.M.: P. 74, T. 98°2; took another drachm. At 4.15 P.M.: P. 78, T. 98°. At 7.15 P.M.: P. 89, T. 98°2; another drachm. At 10 P.M.: P. 78, T. 98°.

March 31.—At 7.30 A.M.: P. 74, T. 98°4; took two drachms. At 9.15 A.M.: P. 76, T. 98°4. At 12.30 P.M.: P. 71, T. 98°2; again took two drachms. At 1.30 P.M.: P. 70, T. 98°2. At 4 P.M.: P. 76, T. 98°; again took two drachms. At 7.30 P.M.: P. 80, T. 98°. At 8.30 P.M.: P. 78, T. 98°; again took two drachms. At 10.30 P.M.: P. 80, T. 98°.

April 1.—The observations of yesterday were repeated, with similar results.

The average of these figures is as follows:—During the morning, P. 73, T. 98°3; at midday, P. 70, T. 98°2; during the afternoon, P. 76, T. 98°1; at night, P. 78, T. 98°. A comparison of these results with those above shows an almost complete identity.

Although the results of the latter observations have been entirely negative, it is some satisfaction to know that they are trustworthy, the greatest care having been taken to secure accuracy. That guarana is a most efficient remedy for sick-headache is beyond all doubt; and that it may prove useful in asylum practice for other conditions of the nervous system is probable enough, and worthy of extended research.

ON THE TREATMENT OF ENTERIC FEVER BY THE USE OF INTERNAL DISINFECTION.

BY STEPHEN SKINNER, M.B. .

IN the following short paper it is my wish to draw the attention of my readers to what I consider the real value of the sulphocarbolate of sodium as a mode of treatment for enteric fever; hoping that possibly some of my brother practitioners may give it a trial, and by their valuable testimony add to or diminish our confidence in the drug.

For it is only by extended use that the true value of a medicine can be properly appreciated. Any private collection of cases, no matter however numerous or favourable they may be, are always open to the objection that they were slight cases, and would in all human probability have got well if left to nature; or to the suspicion that the recorder has been too partial to his remedy, and has, either willingly or in error, aided and abetted in suppressing facts that might possibly put his "favourite" in an unfavourable light. I trust neither of these errors has been mine. I have taken the cases in the exact order in which they occurred—not omitting one—and I think they have all been of the usual type of enteric fever.

In the *Practitioner* for June 1872, I read Dr. Arthur Sansom's able article on "The Antiseptic Treatment of Smallpox," and I was then so much impressed with his views on the subject of internal disinfection that I resolved on giving it a fair trial in every case of smallpox or enteric fever I could meet with. Since then I have been able to treat twenty cases of the latter disease on this principle, and the result has been so satisfactory

that I consider it justifies my confidence in the method. Before I proceed to give an analysis of the cases thus treated, a word as to my mode of administering the drug. I generally commence with twenty grains every fourth hour, as a dose for an adult, and then I gradually increase during the next few days to the full dose of thirty grains. Beyond this I have found that I cannot go without producing cerebral symptoms as the result, which have, however, quickly disappeared upon the diminution of the dose of the drug.

Children, I have found, require much smaller doses in proportion than adults. Where the diarrhoea has been excessive I have combined small doses of Batty's liquor opii sedativus with each dose of the remedy, until the bowels have been quieted.

With the exception of one case, which terminated fatally on the fourteenth day of fever, all the remainder progressed favourably towards recovery.

The length of time the fever lasted in each case appears to me to bear a direct ratio to the promptitude with which the drug was administered. In those cases in which I was able to exhibit the remedy during the period of incubation, the fever disappeared quicker than when it was given on the first or subsequent days of the disease. I have also administered the sulpho-carbolate of sodium to people with premonitory symptoms of typhoid, occurring in four instances in houses where I was already in attendance, and I have been glad to find that the disease has never developed itself; it has, as it were, aborted: after from six to ten days of general discomfort, the bowels have assumed their natural tone and the excrement its normal appearance, the tongue has cleaned, and the patients have declared themselves to be in their usual health again. In these cases I fully believe that if the drug had not been administered, typhoid fever would have been developed.

This is, however, a matter of conjecture only, and beyond proof, and therefore I hope that everyone who has opportunities of observing the action of this remedy on the class of cases alluded to, will give the profession the benefit of their experience.

The following table gives an analysis of my cases:—

No.	Age	Sex	When the treatment was commenced.	Highest temperature and pulse during attack.*	General condition of the tongue.	State of the bowels.	Hæmorrhage.	First appearance of the rash.	Period of abatement of fever.	Remarks.
1	17	F.	Two days before the fever commenced.	106 103-5	Coated throughout with dirty white fur, but tolerably moist.	Opened frequently during first two days, afterwards quiet.	None.	10th day of fever.	17th day.	After the 17th day she progressed regularly towards a perfect recovery. Recovery very rapid.
2	7	M.	Five days before the fever commenced.	112 102	Coated, but moist.	Opened only twice a day throughout.	None.	8th day of fever.	14th day.	Recovery very rapid.
3	25	M.	On the second day of the fever.	120 104-5	Very dry, but not much coated.	Opened very frequently, treated with small doses of opium.	Slight on fifth day, but was not repeated.	10th day of fever.	22nd day.	The recovery was slow but regular.
4	12	M.	On the first day of the fever.	118 103-2	Very much coated at first; after first week, dry and not much coated.	Opened frequently for first four days, afterwards confined.	None.	9th day of fever.	21st day.	The recovery was regular and rapid.
5	35	F.	A week before the fever commenced.	106 101-6	The tongue was thickly coated, but moist throughout.	Opened about twice a day.	Streaks in the faeces on third and fourth days.	7th day of fever.	16th day.	This woman was downstairs on the 21st day.
6	4	M.	Two or three days before the fever commenced.	110 102	The tongue was not much coated.	Opened only once a day after the first four days.	None.	11th day.	17th day.	Recovery rapid.
7	40	F.	On the first day of fever.	112 101-5	Very dry.	Opened very frequently throughout.	None.	12th day.	22th day.	Recovery tolerably quick.
8	25	F.	On the first day of fever.	110 104-8	Very dry.	Opened frequently at first, afterwards quiet.	None.	11th day.	27th day.	This woman miscarried on the sixth day, and lost a quantity of blood, which retarded her recovery.
9	10	F.	Three days before the fever commenced.	112 102-3	Coated, but moist.	Opened very frequently.	Slight on the seventh day.	8th day.	18th day.	Recovery rapid.
10	19	F.	First day of fever.	114 104	Dry; unaturally red.	Opened three times a day.	None.	12th day of fever.	26th day.	Recovery regular, but slow.
11	16	M.	On the first day of the fever.	115 104	Dry.	Diarrhoea moderate after the fourth day.	Very few streaks on the second day.	9th day.	24th day.	Made a good recovery.
12	6 1/2	F.	Not seen until the third or fourth day of fever.	124 105-8	Very dry.	Diarrhoea excessive.	None.	10th day.	28th day.	Made a good recovery.
13	40	M.	On the first day of fever.	110 104-5	Dry, but not coated.	Diarrhoea well marked for the first five days.	A sharp attack on the fourth day.	No rash.		Died on the 14th day of fever. His end was hastened very much by vomiting and excessive tympanitis.
14	54	F.	On the first day of fever.	112 103	Rather dry.	Bowels only moved twice a day after the first three days.	A very slight attack on the fifth day.	8th day	25th day.	Made a very good regular recovery.
15	8	F.	Four or five days before the fever commenced.	116 102-8	Very much coated.	Diarrhoea well marked throughout.	None.	No rash.	19th day.	Made a rapid recovery.
16	31	F.	On the first day of fever.	120 104-5	Dry and coated.	Diarrhoea moderate.	None.	No rash.	32nd day.	Made a good recovery.
17	27	F.	Did not see her until the second or third day.	130 106	Dry and brown.	Diarrhoea excessive.	None.	No rash.	27th day.	Made a regular but slow recovery.
18	44	F.	Three days before the fever commenced.	130 103-4	Moist throughout.	Diarrhoea very moderate after the second day.	None.	9th day.	16th day.	The prostration was very great. Made a very good recovery.
19	38	M.	First day of fever.	115 104	Tolerably moist.	Three motions a day for first week, afterwards confined.	A trace for two or three days.	10th day.	23rd day.	Made a regular recovery.
20	42	F.	A day or two before the fever commenced.	110 102-6	Tolerably moist.	Diarrhoea excessive for the first week.	None.	No rash.	20th day.	Made a tedious recovery. The weakness was very great.

* The temperature was taken at 7 P.M.

ON DIGITALIS IN ACUTE FEBRILE DISEASES.

BY DR. ANSTIE.

IN the following short article I cannot attempt a general discussion of the supposed anti-febrile action of digitalis. The large amount of evidence on this subject which has been accumulating during the last twenty years, together with my own special experiences, would require a more elaborate analysis than could be found space for in the pages of this journal. But it may be useful to draw the attention of English observers, rather more closely than it has yet been drawn, to those objects which there is really some prospect of effecting by means of digitalis in pyrexial diseases.

The special subject which will now be discussed is suggested to us by the appearance of a pamphlet by Dr. T. W. Grimshaw¹ on the action of digitalis on the weak heart of typhus; a paper which contains some remarkable facts, although I am not able to agree with all its conclusions. The question of the power of digitalis as a heart- tonic in the so-called adynamic fevers generally—not merely in typhus—is becoming very important: and the following remarks will be concerned with this alone, to the exclusion of other questions, such as the power of digitalis to lower temperature in acute disease.

I can hardly preface my own observations more effectively than by bringing forward some of Dr. Grimshaw's facts. When it is remembered how strikingly the pulse of typhus fever exhibits the characters of low arterial tension and low heart-force, it must indeed be astonishing, to those who cherish any

¹ Reprinted from the *Dublin Quarterly Journal of Med. Science* for June 1873.

remnant of the classical belief in digitalis as a heart-depressor, to find Dr. Grimshaw giving 1 ounce, and even $1\frac{1}{2}$ ounce of the *infusion* of digitalis every three hours for five or six days together, not only with impunity, but with seeming benefit. Whether the benefit was real or not, at least this is certain, that in nearly all the cases there was a total absence of those symptoms of heart-depression which used to be held up as a bugbear of digitalis treatment; indeed, on the contrary, it is asserted by Dr. Grimshaw, with apparent accuracy, that both the heart-force and the arterial tone were markedly invigorated. And in addition to this, it is to be remarked that but small amounts of alcoholic stimulants were given in any case, and in several no alcohol was administered. There were but two deaths out of twelve cases, and in the whole of the twelve recoveries the patients were adults.

I do not propose to dwell any further on the cases brought forward by Dr. Grimshaw, which have been cited simply as proof of the fact that in typhus, even of severe type, the phenomena of cardiac and vascular depression are relieved rather than aggravated by the use of digitalis in very large doses. Observe, that these cases, in which the *infusion* of digitalis was employed, stand on a very different footing from experiments in which large doses of the *tincture* had been employed: so long as the evidence consisted only of instances in which the latter preparation had been used it was impossible to say how far the result had been influenced by the large quantities of alcohol necessarily given. In Dr. Grimshaw's cases, on the contrary, we have to deal with an uncomplicated digitalis action.

It would be a mistake, however, to limit our consideration of digitalis as a heart-tonic in febrile diseases to the case of typhus. It must not be forgotten that the latter is an affection that runs a brief course, and in which asthenia is not the mode in which death usually occurs. Some of the best authorities on fevers have remarked that the issue in uncomplicated typhus depends upon the intensity with which the nervous system is affected, far more than on cardiac asthenia of local origin; and certainly the characteristic aspect of such cases is pre-eminently that of overwhelming nervous poisoning. No doubt the muscular tissue of

the heart is frequently, if not always, considerably changed, and both Murchison¹ and Buchanan² state, from personal observation, that in fatal cases this affection is not a mere fatty softening, but sometimes, at least, involves more or less elaborate changes of the same kind as those which have been so carefully described by Zenker as occurring in the heart and voluntary muscles of typhoid patients. But on the whole it seems evident that local cardiac changes count for much less in a brief disease like typhus than in a prolonged affection like that form of typhoid in which death occurs from simple heart-failure at an advanced period, and in which "Zenkerism" of the cardiac muscular substance is especially pronounced. It is in connection with this latter form of fever that I have more especially studied the question of weak heart in febrile disease; and before speaking of digitalis as a remedy, I wish to make some remarks upon the clinical facts of the case.

I doubt whether any published account of typhoid fever gives a just idea of the frequency with which sudden failure of the heart—and this alone—is the cause of death in cases of typhoid which run a longer course than four weeks. I do not mean that all these deaths are instantaneous; but the following is a fair average description of what happens:—Either from the original intensity of the febrile attack, or from the existence of severe complications, the heart has been kept pulsating at a rate varying from 120 to 150 or more, under an internal temperature which was always very high, even at the daily minimum, for some four or five weeks. The inflammatory complications, or the intestinal ulceration, however, have fairly subsided, and the pulse now begins to change. But instead of the simultaneous diminution of frequency and increase of arterial tone which would show the passage to convalescence, we remark (if we observe with attention) a very different kind of pulse-change. Careless feeling of the radial pulse might lead one to believe that the pulse, though somewhat irregular, had diminished very greatly in frequency; but a thoroughly skilled finger, or still more surely a sphygmograph, would immediately bring out the fact that many small intermediate pulsations had been over-

¹ On "Continued Fevers of Great Britain," second edition.

² Article, "Typhus Fever," in Reynolds' Syst. Med. vol. i.

looked ; in fact, that the pulse is scarcely, if at all, less rapid than before, but now presents the dangerous quality of varying in force from moment to moment, *and that in an unrhythmical manner*. Nothing can be of worse augury than this—more especially when the sphygmograph shows the combination of rounded apices of the individual pulse-waves with unrhythmical undulation of the general line of the tracing. It is under such circumstances that patients often “sink out” after all active mischief would seem to have ceased. It is very noticeable that such a catastrophe is far more common in adults than in children : the hearts of the latter possess a prodigious power of recuperation, which seems to be reduced to a minimum by the time that the middle age—so dangerous to typhoid patients—is reached. At the latter epoch, also, not merely is heart-failure a too likely occurrence in the later weeks, but this form of death may overtake the patient at an earlier date ; and there seems reason to believe that in such persons the degenerative process goes on with far less interruption from recuperative efforts than in the young.

It is now well known that a very marked rise in the mortality from typhoid is found in the quinquennium immediately succeeding the development of puberty ; and from personal observations I believe that, even so early in life as this, there is a change in the nutrition of the cardiac muscular structure which renders it more incapable of resisting the influence of high temperature and very rapid pulsation for prolonged periods, and which consequently allows, more frequently, the occurrence of death from heart-failure. But the tendency is, doubtless, greatly increased later in life.

I think we are entitled to view the occurrence of muscular heart-failure in acute febrile disease as essentially dependent upon an enforced rapid action, under high temperature, prolonged for a period which is excessive, *in proportion to the vital recuperative power of the cardiac muscular tissue*. In this point of view we should logically place on a line with the effects of the mere prolongation of high temperature and rapid pulsation, in protracted typhoid, the effects of a shorter exposure, to these conditions, of a heart whose tissues had been previously so modified by pathological degeneration as to render their restora-

tion to a healthy state unusually difficult. This principle is very important. We should expect heart-failure to be a very formidable danger in delirium tremens, with high temperature and rapid pulse, if the patient were an old drinker; and, similarly, we should expect that acute diseases, like pneumonia or erysipelas, occurring in persons who had long been addicted to alcoholic excess, would be very liable to terminate fatally in the same way. Nothing can be more certain than that this liability exists; it is the terror of every experienced physician who encounters these acute maladies in old toppers. Such persons cannot endure for more than a very limited period the presence of pyrexia and rapid pulsation; their hearts give way under the combined pressure.

In applying considerations of this kind to the question of digitalis treatment for heart-weakness in febrile disease, it is at least evident that there are very good *prima facie* grounds for expecting valuable results in a considerable proportion of cases. By a strange perversity, which is the more surprising in view of the greatly improved pathological knowledge of the present day, too many teachers of medicine ignore the influence (which cannot be too highly estimated) of the previous condition of the bodily organs of any given patient upon the cause of acute febrile diseases by which he may chance to be attacked. Such an error is to be strenuously avoided by any practitioner who desires to attain a mastery of treatment; and certainly, in no instance should it be more carefully shunned than in the use of digitalis in acute febrile disease.

What is it, in fact, that we expect to gain from digitalis in acute febrile disease? Putting aside the question of reduction of temperature, which we are not now considering, we may endeavour to operate on the heart itself. It matters not whether we adopt the theory of vagus stimulation or that of direct tonic action on the muscular fibre *plus* a stimulation of the vasomotor system, producing tonic contraction of the arterioles; the practical effect at which we aim (and are justified in so aiming by a number of experimental facts) is twofold—the simultaneous slowing and strengthening of the ventricular contractions. It may be asked whether this in itself amounts to any real advantage, unless the course of the disease were

also cut short. I believe that, in regard to the danger of heart-failure, it may be of great advantage. The normal tendency of muscle which is persistently overworked in proportion to its nutrition is undoubtedly towards degeneration; and, certainly, there can be no more decided instance of such overwork than that of the cardiac muscles when they are forced to contract some 130 or 140 times per minute, for a long time together, under a temperature of 103° to 105° or 106° Fahr. Merely to reduce the frequency of these contractions must surely be an important object, for the sake of preserving the soundness of the muscular tissue.

A question of much interest arises whether the impunity with which digitalis has been prescribed in such large doses by Dr. Grimshaw is due to the fact that the infusion of digitalis is a weaker preparation than British physicians have been accustomed to suppose. Upon this point there is a curious conflict of evidence, which has never yet been cleared up. The researches of modern chemists (especially those of Wiggers and of Nativelle) have made it clear that digitalis contains two active ingredients—digitaline¹ and digitaleine,²—of which the former is said to be decidedly the most powerful of the two; but of these only the latter is soluble in water to any considerable extent, while the more powerful principle requires alcohol for its extraction. On the other hand, there is abundant recorded evidence of very powerful poisonous effects being produced by aqueous infusions and decoctions of digitalis.³ In making comparisons between the potency of the infusion and that of those preparations of digitalis which contain the more powerful alkaloid, it is necessary, as already said, to leave the tincture out of the question, since the alcohol in that preparation confuses any experiments which are made, and renders their results altogether equivocal. It remains, then, to inquire whether the powder of the leaves, the alcoholic extract, and the crystalline digitaline itself, can be given in respectively large doses in the condition of a weak heart in febrile diseases. Now, it is remarkable that the so-called “digitaline” of Homolle and Quevenne (a mixture,

¹ Crystalline.

² Amorphous.

³ *Vide* Christison on Poisons, Taylor on Poisons, and many other toxicological works.

in fact, of digitaline with a certain amount of digitaleine and digitalose), the dose of which, as recommended by the inventors, is from $\frac{1}{17}$ to $\frac{1}{11}$ grain, has been given by Trousseau, in uterine hæmorrhage, in as large a quantity as 1 gramme (15·6 grains) in the twenty-four hours; and by many other physicians, in pneumonia, typhoid, &c., to the amount of nine or ten grains daily. And the powdered leaves of digitalis have equally been given, especially by the German physicians, in doses which sound perfectly frightful as compared with the doses laid down by authorities in this country.¹ On the whole, then, it appears at least very probable that there are some circumstances which confer on the organism a large degree of tolerance for digitalis; and, looking to the remarkable experience of Trousseau above mentioned, we get more than a suspicion of the grounds of this tolerance. Trousseau gave as much as 15·6 grains of digitaline in twenty-four hours to a woman suffering from uterine hæmorrhage; and we can hardly doubt that in a healthy person a much smaller dose than this would have caused fatal tetanus of the heart. Good digitaline of Homolle and Quevenne is probably quite fifty times (not 100 times, as the inventors say) as strong as fresh digitalis leaves; and, not to mention the instances—due, probably, to idiosyncrasy—in which as small a quantity as one or two milligrammes (·0156 grain, or ·0312 grain) has produced uncomfortable symptoms, it has very frequently happened that doses twice or three times as large as this have caused nausea and great depression. The true explanation of the tolerance of such enormously greater doses in cases of uterine hæmorrhage must be sought in the modern physiological experiments which have shown that the real action of digitalis on the heart is that of a stimulator, instead of a paralyser, of the cardiac muscular substance; in accordance with which fact one may well suppose that the extremely weakened cardiac muscular fibre of a patient, exhausted by profuse hæmorrhage would sustain and even require a dose of digitalis, merely to support a sufficient amount of contraction to continue life, which in health would fatally tetanise the heart.

The general effect of the various considerations which have now been advanced seems to be this:—that the action of

¹ *E.g.* thirty to fifty grains daily by Wunderlich and Hirtz.

digitalis (a) as a sustainer of exhausted heart-power, and (b) as a restrainer of the rapidity of pulsation, seems to especially point it out as a promising remedy in that very large class of acute febrile diseases in which the greatest danger is failure of the heart before the malady has had time to run through its natural periods of development; and that there are strong grounds for holding this opinion, whether we admit or deny the reduction of temperature which is so strongly maintained by many eminent authorities. On the latter point I cannot but think that judgment should be as yet suspended; not that I deny the fact that febrile temperatures can be reduced by large doses of digitalis, but that a careful review of the literature of the subject leaves me in doubt whether the lowering of temperature is not, in an inconveniently large number of cases, attended with an amount of nausea and depression which more than counterbalances the advantage of the mere cooling. In my own experience, which may have been unfortunate, it has been difficult to frame any rules of dosage and circumstances under which one might with confidence expect a beneficial reduction of febrile temperature. But I can answer for the fact that, even without this action, digitalis has, to all appearances, proved of the utmost value in many cases of acute disease running a protracted course or occurring in subjects whose tissues were damaged by disease or intemperance. And the more I reflect upon the matter, the more I am inclined to trace these benefits to the sustaining action of digitalis upon the enfeebled heart.

THE TREATMENT OF NEURALGIA BY ELECTRICITY.

BY GEORGE M. BEARD, M.D., OF NEW YORK.

ALTHOUGH the subject of electro-therapeutics in general is now exciting great interest in all the great centres of science, there is yet, concerning the application of this agent to the treatment of neuralgia, considerable difference of opinion as well as of practice. The reasons for this difference of judgment in regard to the adoption of this most potent remedial agent to neuralgia are, it seems to me, as follows :—

1. The diagnosis of neuralgia is oftentimes incorrectly made. It is confounded with rheumatism, with spinal sclerosis, with the vague wandering pains of hysteria, and with any number of local diseases.

As I shall aim to show, in the course of this paper, the methods of electrical treatment of neuralgia must be essentially different from those employed for many of the conditions with which it is so often confounded. A form of application which will work well for rheumatism, or for the pains and aches of hysteria, may not only utterly fail to relieve true neuralgia, but may fearfully aggravate it.

2. The pathological seat of the disease in neuralgia is not properly recognised or acted on. Although I am not yet prepared to admit that atrophy of the roots of the sensory nerves is the usual lesion in neuralgia, since the arguments in favour of that view, as advanced by Dr. Anstie, are hardly satisfactory, yet the general idea that neuralgia is a central disease has long been my conviction. It is not inconsistent with this view to suppose that certain forms of neuralgia—as,

for example, some varieties of sciatica—may be in part if not wholly peripheral.

The generally accepted notion that the disease is where the pain is, has led physicians to apply the current solely to the seat of the pain. Logic and experience show that the seat of the disease, the central nervous system, should also be treated.

Then, again, it has not been recognised that the different forms of neuralgia are but manifestations of a constitutional tendency, and should be treated accordingly.

The whole central nervous system is invaded by a mysterious foe that darts out along the great pathways of nerve-force in any direction and where least expected. The special manifestations of the neuralgic tendency are often different at different periods of life.

I have now under my observation a gentleman of middle life, who, in youth, suffered from facial neuralgia; in later years, chronic cervico-brachial neuralgia has perpetually harassed him; during the past year he was prostrated by a terrible attack of gastralgia, and just now he is recovering from an invasion of sciatica. To treat such cases by merely local electrification, whether it be central or peripheral, or both combined, may indeed palliate or relieve; but to expect permanent victory over the neuralgic tendency by merely local treatment where the pain last appears, is unreasonable and usually unsuccessful.

3. The differential action of the galvanic and faradic currents has not always been distinctly kept in view. For true neuralgia in any part of the body, the galvanic current is usually preferable to the faradic. It is not true, however, that genuine neuralgia never yields to faradisation; it does so quite frequently; but everything depends on the *kind of faradic current used*, and the *method of application*. There is as much difference of therapeutical effect in the qualities of faradic currents as generated by different machines, as there is between the galvanic and faradic currents.

A rough, harsh current, such as is generated by Stöhrer's machine, will almost always do more evil than good to neuralgia, especially in the irritation stage. Such a current will pretty surely do harm if it be applied strong and recklessly, and may do no good when applied gently and cautiously. On the other

hand, a rapidly interrupted and pleasant current, such for example as is generated by Kidder's machine, when applied cautiously, may, and in my hands frequently has, relieved and cured true neuralgia.

The physical principles that account for this differential action of the faradic currents from these different machines is somewhat complex and cannot here be discussed. I may suggest that the physiological and therapeutical action of faradic currents seems to vary, not only with the length and thickness of the coils, but with the rate and manner of interruption, and with the details of the construction of the helix. All the leading forms of neuralgia—facial, central, occipital, cervicobrachial, gastralgia, and sciatica—I have treated with more or less success by faradisation with secondary and tertiary currents, from machines such as I have referred to; and yet the galvanic current will oftentimes succeed where the best faradic fails; and even in those cases where faradisation relieves and promises to cure, galvanisation may do the work in a much shorter time.

In regard to the question of the differential value of large and small cells in neuralgia, I may say that the idea that large cells send greater quantity of electricity through the body than small cells is opposed to the teachings of Ohm's law, and has no basis in experience. It is, however, not improbable that large cells, especially those various modifications of Daniell's, by virtue of their greater constancy and the great slowness and uniformity of the chemical changes that take place in them, may be less irritating, and therefore better adapted for neuralgia and other irritable conditions.

The difference in this respect cannot, however, be very great, since many of the greatest triumphs of the galvanic current over neuralgia have been gained by small and inconstant single cell batteries. The larger double cells are more steady in their action, but steadiness may not always be necessary even in testing neuralgia, and it is by no means impossible that in some conditions the very fluctuations of the single cell battery may have a therapeutical advantage. Certain it is, that many of the brilliant results in the treatment of diseases of the central nervous system by electricity have been obtained by portable

batteries, composed of a moderate number of small, single, and inconstant cells of zinc-carbon or zinc-platinum.

The electric moxa—faradisation with the metallic brush—is a method of treatment which in my hands has not been successful. I have seen the method used for sciatica in Berlin and in France, and I believe that it sometimes accomplishes good things; but it is a most atrociously painful procedure, and I rarely find a private patient in America who will endure it, and for this reason I have almost lost the habit of ever attempting to use it.

4. Too strong currents have been used, and too much stress has been laid on the differential action of the poles or of current direction. Beginners in electro-therapeutics make mistakes in many directions, but in no one more than in the habit of using too powerful currents. In many diseases and in many temperaments strong currents are harmful, but they are especially so in neuralgia. Better to use a mild current a long time than a strong current a short time: electro-therapeutics cannot be thus concentrated. To attempt to gain time in this way is cruel economy.

In regard to the differential action of the poles and of the current direction, I admit that of course there is a differential therapeutic, as there is a differential physical and physiological action of the poles, but it is far less marked in therapeutics than in physics and physiology. The importance of direction of the current, whether ascending or descending, has been enormously overrated, not only in neuralgia but in all the diseases for which electricity is employed. Pain is relieved, neuralgia cured, and the nutrition in every respect improved, by the passage of the current through the body by either pole and in any direction, and the differential action of the poles and of the current direction is so slight and so complex that it cannot in the present state of physiological and pathological knowledge be reduced to any formula. I have elsewhere laid down the very general law that in therapeutics the positive pole is less irritating; but even this general statement seems to have many exceptions, in local application at least, for the negative will frequently relieve an irritated nerve as well as the positive, if not better. In central galvanisation and general

faradisation, I use, as a habit, the positive pole in making the application to the head or neck, believing this to be, on the whole, the safer.

5. The treatment has not been used with sufficient perseverance. Perhaps by the use of too strong currents the pain is at first increased; the patient and physician become alarmed and abandon the treatment. This is frequently a mistake; the fault is in the method, not in the medicine. Even when the pain is increased temporarily the permanent effect may be beneficial. Neuralgia is frequently a chronic disease, and like other chronic diseases demands chronic treatment. Neither patients nor physicians should be terrified or discouraged because the first applications appear to aggravate the attacks.

With these general remarks I proceed to speak of some of the special forms of neuralgia.

Migraine.—The prognosis of migraine under electrical treatment is favourable for relief and palliation, but to thoroughly and permanently eradicate the habit is more than we can generally expect. Even relief of the pain during the height of the attack is by no means a uniform result. Here, as everywhere in electro-therapeutics, as much depends on the temperament of the patient, or his general susceptibility to electricity, as on the special disease from which he suffers. The attacks may be forestalled or aborted at the beginning by electrical treatment, just as they may be by bromide of potassium, or sodium, or hydrate of chloral, or gelsemium; but even here there is no uniformity.

In some cases the application of a very mild galvanic current to the head or neck at once causes the pain to depart, and it does not return. In other cases the same treatment does no good, and sometimes intensifies the pain. I have been at different times a victim to migraine, and have tried galvanisation before the attacks, while they were coming on, and at their height, and never with success. I have friends similarly afflicted, whom the same treatment for the same kind of attack always more or less relieves.

I think I have observed that migraine, accompanied with gastric disturbances, is more rebellious than when the disease is confined to the head.

Although electro-therapeutics must be based on clinical ex-

perience, it is yet an advantage to have some general notion of the pathology of the disease that we treat; and when, as in the case of migraine, we cannot demonstrate the precise nature of the morbid changes in the nervous system that give rise to the symptoms, it is yet desirable to obtain, or to endeavour to obtain, clear and correct ideas of the seat of the disease, even though we can go no further. Our method of applying electricity will be guided, though it should not be controlled, by the theories we hold or the facts that we possess, or suppose that we possess, in regard to the pathology of the disease for which we employ electricity.

Without entering into an extended consideration of the nature of migraine—a subject which has been well and variously discussed in a series of articles in the *Practitioner*.—I may simply say, in brief, that I regard the disease as a neuralgia closely related to facial neuralgia and gastralgia; that it may be excited by mental influences, by exhaustion, or, reflexly, by digestive or genital disturbances; that nausea and vomiting are the results and attendant symptoms of the disease, rather than the disease itself; and that the seat of the disease is in the pneumogastric and fifth nerves at their origin.

Acting in a measure on these views, which are based on long study of the affection, I treat migraine by my method of central galvanisation, which consists in placing *the negative pole to the epigastrium (the patient holding it by an insulated electrode), while the positive is applied on the top of the head, over the sympathetic in the neck, and down the whole length of the spine, in such a way as to bring the brain, the pneumogastric, the spinal cord, and all the prominent plexuses of the sympathetic, indeed the whole central nervous system, under the influence of the current.* This method is to be used in the intervals of the attacks, with the view of preventing their recurrence or of lengthening the intervals. During the attack, galvanisation of the brain or of the cervical sympathetic and pneumogastric in the ordinary method may be used for merely temporary relief. Faradisation from the machine, that gives a pleasant current, with the hand as an electrode, sometimes acts as well as galvanisation. In all these applications mild currents should be used—so mild as to be scarcely felt, even when the sponges or flannels are saturated with salt water.

It is not indeed necessary that the current should be felt at all, so long as we are sure it is running, and the current should be gradually put on or off. Acting with these precautions, galvanisation of the brain, or of the *neck*, is an *absolutely safe procedure*.

The mistake of electro-therapeutists is, to apply to the head or neck currents as strong, or nearly as strong, as those that they apply to the muscles in the treatment of paralysis.

During the past few years I have made many thousand applications of the galvanic current to the head and neck of patients of nearly all ages and of every degree of delicacy and susceptibility, without producing any dangerous or alarming effects whatsoever. In two or three instances, through my carelessness in suddenly interrupting the current, unpleasant, though not serious, effects have occurred, which soon passed away.

In using central galvanisation I sometimes move the negative electrode down over the abdomen and the hypogastric region, the positive being on the back, so as to bring the uterus and ovaries under the influence of the current. In cases where there is reason to suspect that the migraine is excited by uterine irritation, systematic electrical treatment of the uterus and ovaries, internally and externally, has been of advantage.

Facial Neuralgia.—This formidable disease yields to electrical treatment only in a certain proportion of cases. The brilliant results recorded by Niemeyer and Reynolds are rare. The milder cases, in patients not too advanced, and brought on by exposure to cold, sometimes are relieved and cured both under faradisation and galvanisation. The more severe and long-standing cases in the very aged may be relieved, but are not usually cured. The exceptions to the statement are, however, sufficiently encouraging to make us try all cases that come under observation. Galvanisation of the affected nerve-branch and of the brain, in various directions, and galvanisation of the cervical sympathetic, are the methods that I employ in this disease. This is one of the forms of neuralgia where it is very easy to aggravate the pain by an incautious use of electricity, and where there is especial need of patience and firmness. The temporary aggravation may be followed by relief, and the patient should not be allowed to abandon the treatment simply because the pain is

made worse during the first week of treatment. Patients should be taught that they must give to electricity the same respect that they give to other remedies, and allow both it and the physician who employs it a fair and honest trial. The chances for permanent cure of facial neuralgia of long standing in the aged are perhaps not more than one in ten, if indeed they are as good as that; but such a chance is enough to stimulate our perseverance in every case that appears.

Gastralgia.—The form of neuralgia in which I have found the best results from electrical treatment is gastralgia. Cases where the pain is prevalent and of a most agonising character have been not only relieved for a few hours or days, but permanently cured, so far as can be seen now, by central galvanisation, which is *par excellence* the method of using electricity in this affection.

The two important facts connected with the prognosis of gastralgia under central galvanisation, judging from experience up to the present date, are that very long-standing cases are cured, and the relief is permanent. Two years ago I treated two patients—both physicians—for gastralgia of the most severe character: both were rapidly relieved and completely cured, and both are well to-day. In one of the cases the disease had existed for fourteen years, the attack coming on periodically about two o'clock every night.

In another patient, who for twenty years had suffered from gastralgia and angina pectoris, the cure after a long course of treatment has also been permanent.

Possibly I have been peculiarly fortunate in my cases of gastralgia; but if I were to generalise from my own observation, and from the observation of some of my friends who have employed central galvanisation in this affection, it yields better and more permanently than any other form of neuralgia.

Faradisation, however administered, does not accomplish all that we desire in gastralgia; it relieves, but does not entirely cure. The cases above referred to had all been treated by faradisation, in one instance with some benefit, but not with satisfaction. Galvanisation of the cervical sympathetic and pneumogastric may be used for gastralgia, but these methods have never been my dependence in any form of neuralgia.

Sciatica.—The prognosis of sciatica under electrical treatment,

however skilfully administered, is not so satisfactory as is the prognosis of several forms of neuralgia. My experience with this disease does not accord with the highly favourable reports which have been given by various writers.

There is a certain proportion of cases of sciatica that yield readily to the soothing influence of the galvanic current, and after a few applications recover entirely; but such cases are not the rule, although the soothing, quieting, relieving effects are so common as to be almost always experienced. The pain gives way to a proper application of galvanism very frequently during the course of the sitting, yet there is pretty sure to be a recurrence of pain in the course of a few hours, and a very long course of treatment is necessary to make a permanent cure, and there is a certain proportion of cases that never recover, however perseveringly the treatment may be carried out.

There appear to be two reasons why sciatica is thus obstinate.

1. The nerve is larger and longer than any other nerve of the body, and the extent of surface to be treated is, of course, much greater than in other forms of neuralgia.

The disease would appear to be in part peripheral, at least in those varieties that are excited by rheumatism, if not in all the varieties. This view is rendered probable by the very great tenderness, and by the fact that pain is excited by pressure, as in sitting, or by any constrained position of the muscles that cause irritations of the nerve.

2. The nerve, especially near its origin, is so deeply situated that only a very small portion of the current, in external applications, can directly reach it.

My method of treating sciatica is to place one pole—it matters not which—on the spine, near the origin of the nerves that supply the lower limbs, and to pass the other slowly and steadily up and down the whole course of the nerve in the leg, giving especial attention to the tender points. Sometimes I place one of the poles in the hollow of the foot, fastening it there by a cloth or band; but I am not aware that there is any advantage in that arrangement.

I use a current that gives a slightly burning feeling without being positively painful. This method of application will relieve for the time, perhaps for several hours, or for an entire night or day.

Recently I have attempted the treatment of sciatica by *electro-puncture*. The needle, either insulated or not, should be bayonet-shaped, so as to go in easily, and should be inserted by preference at some tender point. I generally try to insert it far enough for the point to touch or penetrate the nerve. The moment when it so touches or penetrates will be revealed to the patient by a tingling or pricking sensation through the leg.

I connect the needle thus introduced with the negative pole of a mild galvanic current, two, four, or six cells, while the positive is applied at the back or some indifferent point. The application should not be made long enough, nor should the current be strong enough, to cause much electrolytic action. It is my custom to use this method for a few moments and then allow the needle to remain *in situ*, to resume the external application for a few moments, closing the sitting by again connecting the needle with a mild current. Thus I combine electro-puncture with the old treatment of acupuncture. I am inclined to think that this method of treatment is more rapidly and more permanently efficacious than external applications. It concentrates the current at the point where it is needed, and perhaps may also act as a counter-irritant. It is probably better that the needle should be insulated up to within a short distance of the point, but I have frequently used uninsulated needles.

The great practical difficulty with electro-puncture is that the introduction of the needle is painful and full of terrors for the sensitive patient.

This difficulty may be overcome in various ways: by inserting the needle quickly and rapidly without forewarning the patient, by using a mixture of carbolic acid and ether, equal parts, or ether spray, to produce local anæsthesia.

But there are some patients who so dread the thought of the needle that they will suffer anything before resorting to it. Two cases of this sort have recently fallen under my observation. One of these was a physician, who is himself accustomed to the use of the electro-puncture, and who has had excellent success with it in sciatica. He had become so reduced by the disease, that when I was called in he refused to be treated by electro-puncture.

Reviews.

On Megrin, Sick-headache, and some allied Disorders : a Contribution to the Pathology of Nerve-storms. By EDWARD LIVEING, M.D. Cantab., Hon. Fellow of King's College, London ; formerly Assistant Physician to King's College Hospital ; Examiner in Medicine, University of Cambridge, 1870-71. London : Churchill, 1873.

THERE are two kinds of books to the reviewing of which the critic, after perusal, sits down with an easy and a happy mind. There is the really first-rate book, and there is the book which is so perniciously stupid that it gives one a thrill of joy to execute justice on the offender who has fouled the stream of literature with his muddy discharge. Either of these, we say, it is a pleasing entertainment to criticise. Alas that so much of our labour should be spent upon that large and uninteresting middle class of works which have no distinct savour at all, whether of life or death, of badness or goodness ! It is those that really try the temper and endurance of the reviewer who happens to possess a conscience.

Dr. Liveing's book, we are happy to say, belongs unmistakably to the first-named kind ; it is entirely an excellent work in every regard. Nothing has been spared ; neither painstaking research into the opinions of predecessors and contemporaries, nor diligent and acute clinical observation, nor (above all) the lucid arrangement of facts in a manner which should best display their mutual bearings, and the general interpretation that ought to be put upon the whole. When we add that the language and style are elegant and scholarly throughout, and that a most interesting subject is treated in a manner to heighten rather than decrease its natural attractions, we trust that our readers will perceive that we are introducing them to an intellectual treat such as the literature of practical medicine provides but rarely.

Like so many other physicians who have written valuable monographs, Dr. Edward Liveing has been for years a sufferer from sick-headache, and he has also had ample opportunities of studying it in the persons of relatives and friends. This has led him to pursue the subject *con amore* ; and though we may

regret that the diffidence of which he speaks in his *preface*, and other unavoidable causes, have delayed the appearance of the work for several years, the general result obtained is one which, perhaps, would have been impossible if there had been more haste in production. The book is divided into seven chapters, with an appendix and an elaborate table of cases. In the very first chapter, Dr. Liveing touches the key-note of his subject by showing that megrim is of various kinds, but that in each aspect it shows a truly nervous character, both by its own immediate origin, and even yet more remarkably by its connection, through hereditary descent, with other neuroses. Our readers are aware that the latter subject is one which has particularly engaged our attention; and it was, therefore, with the deepest interest that we followed the author's investigations of this matter. Not only has he illustrated the fact of neurotic inheritance (direct and transformed) abundantly from his own recorded cases, but he has done great service in bringing together isolated observations of earlier writers, now little read, which show that a large body of evidence on this question really existed, unrecognised by the general profession, long before the researches of Morel and Moreau had given a definite shape to the doctrine of the inherited neurosis.

Nothing, in fact, can be more instructive than the research into the first origin, the rudimentary traces, of a great doctrine like that of the inheritance and transformation of the neuroses; more especially when the author of such a research has at once the patience and the impartiality of mind to enable him to estimate with correctness the various influences that have tended to modify and obscure the pathological and practical conclusions which must otherwise have been arrived at by the authors of the original observations. Dr. Liveing has resuscitated for us a number of most suggestive observations of various authors (beginning with old Willis in the 17th century) who had had the hereditary and also the neurotic characters of megrim and its allies forced upon their attention, and who have very plainly acknowledged the importance of these facts, but have in almost every case been diverted from the fruitful path of inquiry thus opened up by the tyrannous influence of prevalent pathological theories.

He passes in review the various pathological doctrines which have been broached from the time of Willis (1674) up to the present day: enumerating (1) the doctrine of biliousness, (2) that of sympathy and of eccentric neurosis, (3) gastric megrim, (4) uterine megrim, (5) ophthalmic megrim, (6) modern reflex and inhibitory theories, (7) determination of blood to the head, (8) congestion of the brain, (9) Dubois-Reymond's theory of vascular spasm, (10) Möllendorff's and Latham's theories of

vaso-motor paralysis; and shows, as we think very clearly, how all of these doctrines have been the result of exclusive attention to a particular set of phenomena, owing to preconceived notions which precluded a broad and impartial view of the clinical, and especially of the family, history of the disease. The theory which he has himself adopted is, that the whole group of disorders classed under the name of megrim and sick-headache have for their essence the production of "nerve-storms"—that is, explosive displacements of nerve-force, in a territory of the central nervous system, which is hereditarily weak and susceptible; that this territory, taken in its full extent, includes the optic thalami and the medulla oblongata; but that, while the former of these may be supposed to be engaged in those forms of the disease which are attended with peculiar visual phenomena,—with emotional disorder, with numbness and tingling of the extremities, disturbance of the speech-faculty,¹—only the medulla oblongata may be affected in those cases in which there is merely sick-headache without any of these other phenomena.

As regards the probable seat of the disease, we are in entire accordance with Dr. Liveing, and it may be remembered that we ourselves have independently expressed the opinion that the medulla oblongata must be the part essentially affected in ordinary sick-headache, on the double ground of the anatomical connections between the roots of the trigeminus and vagus which are situated therein, and of the remarkable family connections of migraine, and its frequent interchangeability with other disorders, especially asthma, angina pectoris, and gastralgia, which could hardly have their common meeting-ground in any other part except the medulla oblongata. And we are inclined to agree with Dr. Liveing as to the inclusion of the optic thalami in the (rarer) cases where visual, emotional, anæsthetic, or aphasic symptoms are present. We are also in full accord with him in rejecting totally the idea (recently advanced, in different forms, by Dubois-Reymond, Möllendorff, and Latham) that vaso-motor changes stand in the relation of cause to migraine. We can verify his statement as to their marked inconstancy of the vaso-motor phenomena, which are very often quite absent, and which, when present, show no uniformity in time or manner of appearance. And as regards the seat of the vaso-motor disturbances in those instances in which they undoubtedly do occur, we think with Dr. Latham that it is more reasonable to believe that the higher vaso-motor centre, suggested by Möllendorff as probably existing in the cerebral peduncle or the optic thalamus, is engaged, than that the vaso-motor fibres of the great sympathetic are concerned in the matter.

¹ Through the connection of the optic thalamus with the corpus striatum.

As regards the theory that migraine and its allies are examples of "nerve-storms," we would remark as follows. We have no objection to the use of a phrase which certainly very happily describes the external characteristics of the clinical phenomena. We grant that there is much in the observable series of events to suggest an accumulation of nerve-force previous to the paroxysm, and its discharge during the time of the attack, with the consequent resumption of a stable equilibrium during the intervals, or until the new "storm" begins to brew. But we cannot help wishing that the author had ventured a little further, and suggested at any rate his own idea as to the general nature of the corresponding nutritive changes—for such there must surely be—in the physical seat of the malady. Our own expression of a rhythmically recurrent "molecular atrophy" has been freely objected to: and yet it is difficult to see what general kind of cause can be predicated as likely to start these various forms of disturbance of tension-equilibrium in the medulla and mesocephale except a distinct weakening of nutritive action, *i.e.* if we agree with Dr. Liveing that the whole group of allied neuroses of these parts takes its origin always or almost invariably under circumstances when the nervous centres are especially feeble and delicate. We may gather, however, from Dr. Liveing's generally very excellent chapter on treatment, that he seems to hold the doctrine that there is so real and tangible an accumulation of force in the affected centres, that we ought expressly to endeavour to divert it into other channels: it is in this way that he explains, not merely the relief which has occasionally been produced by the intercurrent of a new and violent emotion—a therapeusis which is not available in ordinary practice—but also the well-known beneficial influence of sustained muscular exertion (short of prostrating fatigue).

For our own part, we have always felt inclined to look upon the effects of well-directed muscular exertion in mitigating the paroxysmal condition in asthma, migraine, epilepsy, and the like, as simply produced by the improvement in general constitution which they tend directly to promote.

As regards the general character of the treatment—prophylactic and curative—which Dr. Liveing propounds, we have only to express a thorough approval; with the one reserve that we wish that it had fallen within his opportunities thoroughly to try certain remedies which he appears to have scarcely sufficiently appreciated. We think he should certainly have tested the value of galvanisation, a very important matter as to which (in the case of megrim) there is much dispute among good authorities. Arsenic, again, he scarcely mentions, save in connection with the malarial form of megrim: but certainly, this is to give a far too restricted idea of its value. And

with regard to bromides of potassium and ammonium, there is yet much that we should have liked to hear him discuss; in particular the degree of its relation to the dependence of megrim on conscious or unconscious sexual excitement, and also the exceedingly important matter of dosage. We should particularly like to know whether Dr. Liveing employed it in the full doses (30 to 40 grains thrice daily) in which it is now mostly used in epilepsy; our own experience has shown that the true value of the drug for migraine can only be appreciated by one who has employed it systematically in this manner.

We close this hasty and most insufficient notice of a remarkable book, by again expressing our hearty admiration of it. No monograph of anything like the same value has been written in the English language, so far as we are aware, of late years; and we can only hope that every medical man will read it, and profit, not merely by the mass of practical information that it contains, but still more by the example which it affords of painstaking and conscientious scientific labour.

Treatise on the Continued Fevers of Great Britain. By CHARLES MURCHISON, M.D., LL.D., F.R.S., &c. &c. Second Edition. London: Longmans, 1873.

THE appearance of a second edition of Mr. Murchison's great work calls for, at any rate, a brief notice. Even this might seem superfluous, considering that everyone will at once be anxious to read the book for himself; but we cannot deny ourselves the gratification of introducing it to our readers with a few words of remark.

The present edition of Dr. Murchison's book is the result of greatly increased experience in fever—including the treatment of nearly 15,000 additional patients at the London Fever Hospital. We believe there are few physicians who have ever preserved cases so systematically, and subjected them to such careful analysis, as Dr. Murchison has done, and he has rather become more than less diligent in recent years. Of forty-four detailed illustrative cases contained in his first edition, five have been expunged, and fifty-four fresh cases added; besides which the new volume deals much more fully and exactly than its predecessor with the question of temperature in the different forms of fever. It need not be said that Dr. Murchison has also taken advantage of every observation which has been made by others during the last eleven years; and he has displayed much skill in assimilating the results obtained by fresh observers. The general result is that we obtain a volume, large, indeed, yet not much larger than the first edition, and one which never for a moment suggests the idea of padding, but rather that of severe concentration.

Clinic of the Month.

Treatment of Acute Phthisis.—In a clinical lecture on acute phthisis, Dr. Handfield Jones, after criticising Niemeyer's directions, states that his experience of the effects of hospital residence on phthysical patients is sadly unlike Niemeyer's. Real improvement is quite the exception; deterioration rather the rule. "Commend me, if I had phthisis, not to Niemeyer's treatment of lying in bed with leeches or poultices on my chest, but to Sydenham's, of daily riding on horseback, which he says is all in all." According to Sydenham, if you do this you may neglect the rules of diet and deprive yourself of no sort of meat or drink. Dr. Bennett has known several young men on large sheep farms in Australia cure their tuberculous lungs by eating fat mutton and galloping about on horseback. He says: "It is now twenty-five years ago since I became convinced of the injury of shutting up patients in their rooms during winter and regulating the temperature, as was formerly the custom. A young man with cavities in his lungs, who had borne confinement in this way tolerably well for a winter, found it so irksome on a second trial, that on one occasion he went out and walked to the top of Arthur's Seat. Instead of being worse, he ate his dinner that day with appetite, all his symptoms were moderated, and under the combined influence of pure air and exercise he ultimately worked out a perfect cure, and is now alive and in good health." Dr. Stokes's case of recovery under a prolonged course of duck-shooting, involving frequent wading in water, is to the same purpose. No doubt there is risk of exasperating pulmonary inflammation by exposing phthysical patients to damp chill air, but they will not escape catarrhs (influenzal) by any amount of confinement to warm rooms, and they certainly suffer injury by breathing deteriorated air. On the whole, as it has been well said, there is more danger in staying in than in going out of the house. (*Medical Times and Gazette*, July 26, 1873.)

Extra-uterine Foetation simulating Ovarian Dropsy.—Mr. Jon. Hutchinson gives an account of a case of this descrip-

tion, which occurred in a married woman, aged 34. She had a large tumour in the abdomen, which fluctuated freely in all directions, but felt firmer in some parts than in others, and especially so on the right side. It so far differed from ordinary ovarian dropsy that it did not pass into either loin, but occupied the middle of the abdomen. Its shape was not unlike that of a pregnant uterus. The patient believed it began on the right side: menstruation had been regular for the six months, but prior to that it had been suspended for nine months, during which she thought she was pregnant. A preliminary tapping was resolved upon, the trocar being introduced two inches below the umbilicus. A dark brown fluid like linseed-tea escaped, and though previously some doubt had suggested itself to Mr. Hutchinson's mind as to whether it might not be a case of superfœtation, he now fully believed it to be an ovarian cyst. The stream of fluid became irregular before any material diminution in the size of the tumour had been effected, but this was attributed to its multilocular character, and it was considered to indicate the propriety of resorting to iridectomy. Immediately after the operation symptoms of peritonitis set in, and on the following day she had high temperature and great abdominal tenderness. Death ensued at the close of the third day after the operation. On post-mortem examination the tumour was found everywhere adherent to the abdominal wall, with evidence of acute peritonitis. On cutting into it, a foetus, which, however, did not appear to have been long dead, was found. Mr. Hutchinson proceeds to make some practical observations, and concludes by stating that extra-uterine foetation cysts ought not to be meddled with in any way, either by puncture or incision, until suppuration has occurred and an abscess or fistula has been formed. (*Lancet*, July 19, 1873.)

Cases illustrative of Cerebral Pathology.—Under this title Dr. Hughlings Jackson gives some most interesting cases of cerebral tumour. In the first case, a woman servant, aged 45, had convulsive seizures, which had commenced three years previously. She then had severe pain in her head in the morning, and she felt exhausted, but these symptoms passed off, when she threw up a little clear water. The fits, which were epileptiform and commenced as before, recurred at intervals of about two months, but she had numerous partial seizures. There was no optic neuritis. She was very weak-minded, her memory was bad, and she spoke thickly. Death occurred suddenly. After death, a tumour the size of a slice of chestnut, about one-third of an inch thick, was found attached to the dura mater, and projecting into the lower part of the transverse frontal convolution. It was composed of fusiform

cells, and there was reason for believing that it was of a syphilitic nature.

A second case was that of a boy, aged five, who was brought to the hospital for epileptiform seizures. The history of the case showed that he had had different kinds of seizures at different ages. The first kind occurred when he was ten months old, and did not recur after the gums had been lanced. In the second kind the right leg would "jerk," and the working would go up the right side and affect the right arm. These occurred at the age of two. In the third variety, which was recent, the right side of the face was affected and speech was lost, but there was no unconsciousness. Sometimes he missed a day or two, and sometimes he had them all day long. The speech was affected, and the right arm was useless for five minutes after the attack, though there was no spasm in it. He had double optic neuritis. Soon after the head began to enlarge. From these facts Dr. Jackson diagnosed the existence of a voluminous tumour or other adventitious product on the left side of the cerebrum, which might either be an abscess, rendered probable by the child's having had discharge from the ear, or hydatids. It proved, from examination after death, which was accompanied by pain in the head and convulsions affecting both arms, to be a large scrofulous tumour, weighing eleven ounces. (*Medical Times and Gazette*, July 12, 1873.)

Pure Milk.—A part of the mortality of infants is owing to the impurity of milk. Mr. Gail Borden has ascertained that a quart of milk, measured and weighed on delicate scales, is equal to 2 lbs. 2½ ozs. The tests were made with different samples of milk at different times, but without materially altering the weight. Mr. Borden has adopted the above as a true weight of a quart of milk having a fair average quality. Hence any person who buys milk may determine by weight, with satisfactory accuracy, whether he receives a quart when he is required to pay for that quantity.

Condensed milk may be often used with advantage in preference to that from milkmen in cities. About this, we learn from a French source that the question whether condensed milk has been prepared from skimmed milk, or from milk which retains all the natural cream, may be answered by ascertaining how much fat is contained in the condensed milk. One hundred cubic centimetres of *unskimmed* milk contains from three to four grammes of fat; one hundred cubic centimetres of *skimmed* milk contains only about one gramme of fat. If milk be condensed four times—that is to say, if into one volume of the solid or semi-solid condensed milk in the tins, there have been concentrated the nutritive ingredients of four

volumes of milk,—there must be four times as much fat in the one volume of condensed milk as there was in the original milk from which it was made. If the original milk had been unskimmed milk, there should be in one hundred cubic centimetres of condensed milk from twelve to sixteen grammes of fat. (*Medical Press and Circular*, July 19, 1873.)

Therapeutical Uses of Iodoform.—Dr. Purdon remarks that during the last three years iodoform has been brought prominently under professional notice in France and the United States, and he has himself employed it with considerable advantage in various affections. It possesses not only tonic and stimulant, but also alterative properties. The dose is from one to three grains, given in the form of a pill. Externally, it may be applied as an ointment in the proportion of one drachm of iodoform to the ounce of lard, a few drops of rectified spirit being first added to dissolve the iodoform. This ointment will be found useful to relieve the pain of cancerous sores; fissures, especially of the rectum; ulcers; and to allay excessive pruritus of the skin, as in prurigo and scabies. For a vaginal suppository, iodoform has been recommended by Dr. E. Cutler, of Boston, United States, in painful uterine diseases. When introduced into the rectum as a suppository, it exercises upon the sphincters a local anæsthetic effect, so that defæcation has been performed unconsciously. In hæmorrhoids and tenesmus it is useful. Moutre's formula is:—Iodoform powdered, gr. xx; cocoa butter, 3j: melt, and make into six suppositories. As a disinfectant its power has been asserted by Righini. Certainly the odour is powerful and permanent, and may be compared to that of saffron. For ulcers, the iodoform ointment is serviceable, especially when they are painful and indolent. Moreover, Dr. Izard has experimented with iodoform in the treatment of venereal ulcers—such sores it heals and cicatrises rapidly. According to the same author, if iodoform be sprinkled upon a soft chancre, it heals quickly and without pain. Moreover, it arrests the progress of phagedæna. M. Demarquay thinks highly of this remedy, whilst Dr. Stiles recommends it as an addition to the ordinary plasters and ointments for syphilitic periostitis. Dr. Gamberini, at the hospital of St. Opsola, Bologna, treats all cases of soft chancres with iodoform—two drachms and a half to an ounce of glycerine. This remedy he has found highly successful. For burns, scalds, and painful boils, the application of an ointment containing thirty to forty grains to the ounce is recommended as a good anæsthetic. Internally, Dr. Purdon finds that a grain of iodoform with two grains of reduced iron, given in form of a pill twice or thrice daily, a most excellent remedy for neuralgic and chronic rheumatic affections. In scrofulous

complaints, such as glandular enlargements, it may be prescribed both locally and constitutionally; and its benefit will not be considered surprising when we remember that iodoform contains 90 per cent. of iodine. Dr. Purdon states that in a case of a young lady of sixteen, who was suffering from an enlargement on one side of the neck, and who had gone through the usual routine treatment of iodine and its various preparations, he prescribed iodoform and iron and iodoform ointment. The affection is now rapidly disappearing. The following are some of the diseases in which he has given iodoform a trial. At the Belfast General Hospital, in several cases of phthisis, in the following way:—Iodoform, sixteen grains; essence of aniseed, one drachm; cod-liver oil, eight ounces: mix; dose, a tablespoonful twice daily one hour after food. Also in neuralgia. Given with iron, in chronic rheumatism, chlorosis, anæmia; and as a suppository in the vagina, in cystitis, in the female. At the Belfast Hospital for Diseases of the Skin, iodoform has been prescribed locally in prurigo, in pruritus ani, in cancerous ulceration, and painful ulcers of limbs. In phthisis, the remedy in second stage of the disease checks the night-sweats and relieves the hacking cough. Moreover, the cod-liver oil agrees better with the stomach when thus combined. In neuralgia it is often curative when other remedies have failed. It acts better when combined with reduced iron. Although not to the point, it may be interesting to conclude this notice by remarking that Dr. Dupré, of the Westminster Hospital, found in the course of his experiments on elimination of alcohol, that after six weeks of total deprivation from alcoholic drinks, and even in one individual who completely abstained from alcohol, the urine contains sometimes a substance which is not alcohol, but which presents the characters by which we generally recognise alcohol. This body passes over among the earliest products of distillation, gives acetic acid on being oxidised, reduces bichromate of potash when diluted sulphuric acid is present, and its aqueous solution has a lower density than water. It furnishes iodoform, and exists in the urine in a very small quantity. (*Dublin Journal of Medical Science*, June 1873.)

Treatment of Hydrocele by Electricity.—Dr. Rodolfi, of Brescia, describes a method which he has adopted of treating hydrocele. Referring to the plans followed by other surgeons, he says that Pecchioli inserted needles in the scrotum; Castelnovo, Schuster, Blandin, Porta, Gamberini, and others, employed the continuous current, inserting the rheophores at two distinct points of the tunica vaginalis; Burdel and Destanche, after introducing needles, placed them in communication with the induced current; and Michel and Pétrequin simply applied

the rheophores to the scrotum. He believes that, though these methods of treatment have been followed by some success, they are not calculated to supersede the use of injections of iodine or of alcohol, drainage, &c. They do not carry out the generally recognised indication, to produce sufficient inflammation of the tunica vaginalis to induce adhesion and obliterate the cavity. Dr. Rodolfi states that he has gained this object in the following manner. The hydrocele having been tapped, and the contents drawn off, a copper probe, about eight inches long, covered with an isolating material, except at the two ends, is introduced through the opening made in tapping. The outer end is placed in connection with a copper wire covered with silk, which is attached to the negative pole of a Grenet's three-pile battery, or a Bunsen's four-pile, charged with a solution of bichromate of potash (water 800 parts, bichromate of potash 50 parts, sulphuric acid 50 parts). The positive pole, by means of a wire attached to the rheophore, with a sponge dipped in solution of chloride of sodium, is applied to the scrotum, near the inguinal region. The operator then touches successively with the point of the probe the whole internal surface of the tunica vaginalis, moving the positive pole at the same time. The application should be made for six minutes in a child, ten in an adult, twelve in an old person. The number of piles used should be two for children, three for adults, and four for old men; and care should always be taken to test the activity of the instrument by the galvanometer. The operation generally causes little or no pain. After the operation, a diachylon plaster is placed over the seat of the puncture. In children, some swelling of the testis and redness of the skin follow in eight or ten hours; in adults, the local reaction appears later. The swelling is painful on pressure only, and subsides in ten or twelve days. It is not attended with fever or loss of appetite. For some days, Dr. Rodolfi applies linseed-meal poultices; after which, he uses frictions of mercurial ointment with belladonna. He does not report any cases, as, he says, he wishes to have time to prove whether the cures are permanent before doing so: but in the meantime he begs surgeons to make a trial of the method he describes. (*London Medical Record*, No. 25, 1873.)

Extracts from British and Foreign Journals.

Comparative Therapeutic Value of the Salts of Protoxide and Sesquioxide of Iron.—Iron, says Mr. J. Creuse in a paper read before the College of Pharmacy of the city of New York, has been used in medicine from times immemorial. Metallic iron, green copperas, iron rust, carbonate of iron, bole Armenia, are mentioned by the oldest writers on medicine and pharmacy; but in former times little importance was attached to the peculiar form in which iron was administered. Some fifty or sixty years ago, a decided preference began to be shown for metallic iron, finely comminuted, and for the protosalts of iron. It was thought then that the easy solubility of those preparations in the stomach was a great advantage, and that theory gave rise to a number of officinal remedies like iron reduced by hydrogen: Vallet's mass, protiodide of iron, &c. Of late years, however, especially since the discovery of the citro-ammoniacal pyrophosphate of iron by Robiquet, the salts of the sesquioxide of iron have been steadily growing in favour. It has been argued with reason that since iron in the human economy is invariably found in the shape of sesquisalts, such compounds should be preferred to all others, whenever iron is indicated. It is always in the form of sesquisalts that iron exists in all vegetable and animal substances which compose human food, and metallic iron or its protosalts cannot be mixed with the simplest aliments without completely decomposing them. Protoxide of iron is as unyielding as it is unstable; when you have combined it with strong acids, you can go no further with its salts, you can make nothing of them, not even an alum. Sesquioxide of iron, on the contrary, is a perfect Proteus; sometimes a base, sometimes an acid, it is always ready to enter some combination or other on the slightest provocation. Mr. Creuse has himself demonstrated that nearly all the insoluble sesquisalts of iron can be combined with the alkaline citrates, forming soluble and tasteless compounds to which he gave the name of quadruple citrates; and more recent experiments have shown him that other vegetable salts besides the citrates possess also

the same property, and that not only the insoluble but also the soluble sesquisalts of iron could form similar combinations. He therefore lays down the general rule that all the salts of the sesquioxide of iron without exception, soluble or insoluble, form combinations with all the alkaline citrates, tartrates, and oxalates; such combinations are invariably green, whatever may be the colour of the iron salt: they are all soluble in water, and nearly insoluble in alcohol; they are all free from ferruginous taste, all perfectly stable and miscible with preparations of Peruvian bark without decomposition. In all of them the presence of iron is so disguised as not to be detected by chemical reagents, unless after the addition of strong acids or of sulphuretted hydrogen, both of which destroy the combination. Mr. Creuse then proceeds to describe the mode of manufacture of the tasteless combinations of the alkaline citrates with iodide, chloride, sulphate, and nitrate of iron. (*Pharmaceutical Journal*, No. 153, 1873.)

Diphtheritic Endocarditis.—Prof. Eberth states that in his researches on bacteritic mycoses, he has observed a special disease of the cardiac valves in pyæmic subjects, and considers that this is due to the small organisms which he has recognised as exciters of secondary suppuration. The disease presents the same characters as ordinary rheumatic endocarditis, and its seat is identical. Under the microscope the deposit appears to be chiefly made up of fibrin threads with large quantities of sphærobacteria, resembling those found in the diphtheritic condition of a wound and in embolia of the kidney. That the bacteria developed in the blood should congregate together and remain adherent to somewhat roughened surfaces of the heart is no more surprising than that the colourless corpuscles should be similarly found to form large coagula, and Eberth is therefore not inclined to agree with Hüter, that they have been transported in mass from the primitive focus of pus. Eberth does not agree with Klebs in regarding the diphtheritic fungus as a separate species, and in holding that both septicæmia and pyæmia proceed from another fungus, the *microsporon septicum*, but thinks that this fungus is identical with the microsphæra of diphtheria. Eberth, however, considers a distinction may be drawn between the septicæmic and the pyæmic diseases, since under the former are included those disturbances which may arise from chemical poisons, whilst the latter are caused only by bacteria. He agrees with Hüter in attributing great importance to diphtheria in the production of pyæmia, and the malign ulcerative endocarditis he describes in this paper he regards as strong corroborative evidence of his view, since it is quite independent of traumatic diphtheria, and constitutes a perfectly independent process; pre-

senting, however, all the symptoms and characteristic morbid changes of pyæmia. (*Virchow's Archiv*, lvii. 2, 1873.)

Conium in the Treatment of Insanity.—Dr. Daniel Kitchen, assistant physician to the New York State Lunatic Asylum, states that during the past eighteen months he has been pursuing special investigations with conium in the treatment of insanity, aided by the thermometer and sphygmograph. The preparations used were the succus conii, an imported article prepared by Ransom and Co., and the fluid extract, made by Squibb, of Brooklyn. The dose of the succus which will produce the physiological action is from a drachm to an ounce, according to the motor activity of the patient; men require larger doses than women. The dose of the fluid extract is from twenty minims to a drachm; twenty minims of the extract prepared by Squibb is equal to about a drachm of the succus conii. Some preliminary experiments upon a healthy person showed that in these doses conium lowers both the temperature and the pulse without any apparent effect on the respirations. In the investigations carried on in the State Lunatic Asylum its effects were carefully observed in 150 different patients, embracing cases of mania, melancholia, and hysteria. The following physiological effects are observed in from ten to twenty-five minutes after a full dose is taken:—1, Suffusion of the eyes and injection of the conjunctivæ; 2, giddiness and sensation of weight along the orbit; 3, dimness of vision and dilatation of the pupils; 4, inability to sustain any mental effort; 5, languor, muscular weakness, with a strong desire to assume a recumbent posture; 6, a dragging sensation in the limbs; 7, pulse and temperature lowered; 8, gentle glow of perspiration over the whole body; 9, usually in half an hour the patient is asleep. It appears that upon the motor centres in the brain Dr. Kitchen has failed to discover any direct hypnotic effect, as in chloral hydrate, yet sleep follows very rapidly, as it almost always follows muscular relaxation, and in a natural way. When large doses are given the action upon the muscles is so quick that the patient is scarcely able to prevent himself from falling by supporting himself with his hands. Its effect is the counterpart of that of strychnine, quieting and conserving the nervous energy, and leaving the muscles to sink into rest. Dr. Kitchen has frequently observed the strong and powerful man, in mania and melancholia, after taking a full dose become quiet, and this state is soon followed by prolonged sleep, from which he awakes much refreshed. Full doses, however, must be given, and the preparation must be good. Dr. Kitchen administered it in eleven cases of epilepsy of long standing, complicated with dementia. The fits were lessened somewhat in number and

severity, though none were entirely relieved. In several cases of facial erysipelas with great restlessness, while bromide of potassium proved of little use, conium relieved pain, and sleep followed. According to Professor Mitchell, the addition of half a grain of blue pill has a most salutary effect. A few cases of sciatica and of migraine dependent on dysmenorrhœa received great relief from conium. In hysteria with epileptiform convulsions, conium given in full and repeated doses afforded much benefit. An important point in respect to conium is, that if it does no good it does little or no harm, as it has but little appreciable effect on any of the secretions; and as it does not act on the brain, it may be safely given in all febrile cases. Dr. Kitchen gives an appendix of twelve cases, showing its effects in various forms of disease. (*American Journal of Insanity*, No. 4, 1873.)

Inequality of the Pupils in Unilateral Affections of various parts of the Body.—A paper on this subject appears in *Allgemeine Wiener Med. Zeitung*, by Dr. F. J. B. Roque. He observes that several years ago Baillarger called attention to the dissimilarity in the size of the pupils that occurs in general paralysis. He regarded this as a lesion of mobility that was of special importance in doubtful cases. This symptom has been justly recognised as a valuable sign by all writers at the commencement of general paralysis. M. Roque finds, from the observation of a large number of cases, that in a great number of unilateral diseases the pupil of the affected side is dilated. According to Baillarger, the difference in size is only perceptible when both pupils are dilated; when they are contracted, no difference is perceptible; it is due, therefore, not to paralysis of the dilated pupil, but to unusual contraction of the other, which remains contracted when the patient is turned from the light. Hence it appears that in general paralysis the pupils contract unequally. On the other hand, in unilateral affections of the thorax and abdomen or of the extremities, the pupils dilate unequally, the inequality disappearing when they contract. In following out his observations, M. Roque rubbed a little belladonna ointment on the forehead of those children who were suffering from pneumonia and pleurisy, and then found that a greater dilatation of the pupil on the diseased side than of that on the opposite side occurred. Considerable difficulty occurs in applying the ointment exactly to the middle line, but when this was accomplished properly, the pupil of the affected side dilated more than the opposite one. Electricity applied to the cilio-spinal region also affords a very good means of effecting the dilatation of the pupils, and of estimating which dilates most. That of the affected side will always be found to dilate most

freely. When two different diseases are present on the two sides—as phthisis on the one and pneumonia on the other—the pupil of that side on which the disease is most acute dilates the most. And again, another conclusion is that when two chronic affections are present on the opposite sides of the chest, the pupil will dilate most on that side on which the glands are affected. (*All. Wien. Med. Zeit.*, No. 26, 1873.)

Treatment of Spermatorrhœa.—In the last part of this journal some observations made by Dr. Mallez on this subject were given. A second lecture upon it appears in a later number of the *Mouvement Médical*. In this M. Mallez states that his experience in regard to electrical currents has been in favour of continuous currents, since, in certain cases of well-marked local atony, a prostatic-rectal excitation may be produced by the use of interrupted currents. The continuous current employed should be as weak as possible, so that sensibility and contractility may only be awakened progressively. In one case where this prudent method of proceeding was departed from, M. Mallez saw, after eight days of localised faradisation, the involuntary discharges of spermatic fluid undergo increase. The most frequent and general indication in spermatorrhœa is to subdue inflammation of the prostatic portion of the urethra, and to diminish its sensitiveness. The best means of effecting this are, in the first place, the introduction of bougies, as in the preparatory treatment of lithotrity, but with this difference, that a longer interval must be allowed to elapse between each operation, lest the reverse result to that hoped for be obtained. The ointments containing belladonna, or morphia, or iodine, frequently recommended to be smeared over the surface of the bougie, are utterly valueless. M. Mallez has, however, observed benefit result from the injection of carbonic acid. The mode in which this is effected is by the use of two flasks, one containing hydrochloric acid and the other fragments of marble, united by a piece of india-rubber tubing; a second piece of tubing, having an elastic ball in which the carbonic acid is closed up, is connected with a catheter, a stopcock regulates the supply, and the part to which the stream of the gas is applied is determined by the depth to which the catheter is introduced into the urethra. M. Mallez has a high opinion of suppositories, and recommends one composed of hydrochlorate of morphia, 8; powder of datura stramonium, 8; and cocoa butter, 40 grains. He also finds suppositories made of gum, glycerine, gelatine, and some active agent, as extract of opium, extract of belladonna, or hydrochlorate of morphia, a very useful means of allaying the excitability of the parts. Suppositories with iodoform may also be prescribed. Laxatives and emollients are indicated, since constipation provokes involuntary discharges, either in conse-

quence of the irritation that such condition excites in the lower part of the large intestine, or by the efforts to defæcate that it occasions. Purgatives should be given when the accumulation of fecal matters has lasted more than a day or two; antispasmodics—as musk, castoreum, assafoetida, eucalyptus, bromide of ammonium—may also be prescribed; and lastly, anthelmintics, when there is any suspicion of the presence of worms.

Thus the treatment of spermatorrhœa, instead of consisting simply of cauterising the interior of the urethra with nitrate of silver, should really be directed to fill the two following indications: first, to increase the cerebro-spinal resistance, by all means giving tone to the nervous system; and secondly, by diminishing the local excitation of the sensory nerves of the genital organs, to prevent the too rapid action of these excito-motor nerves. (*Le Mouvement Médical*, No. 27, 1873.)

Treatment of Angina Diphtherica.—Dr. Lolli, of Trieste, states that for some years past he has constantly employed the following treatment in cases of diphtheria, and with very satisfactory results. In the first place, he never uses any kind of cauterisation, nor any depletory means, as by the withdrawal of blood, purgatives, or emetics, except the latter in some very exceptional cases. Secondly, he pays especial attention to the activity of the skin, and in every way endeavours to excite it, as by keeping the patient warmly covered in bed, by the application of warm poultices and mustard-leaves; and this not only at the commencement of the affection, but till every trace of it, both general and local, has disappeared. Thirdly, he uses, both internally and externally and by inhalation, the ingredients of the following mixture, in various proportions and degree of concentration:—

℞ Aquæ calcis, uncias quatuor ad libram unam.

Liquoris ferri sesquichloridi, scrupulum unum ad drachmas duas.

Acidi Phenici, granum unum ad scrupulum unum.

Mellis rosarum, unciam unam. Misco.

This mixture, well shaken up, he applies every two hours to the fauces, and gives a teaspoonful, in weak solution, also every two hours alternately. Fourthly, he orders nourishing food in moderate quantity, and warm or cold drinks at the option of the patient. Before taking any food or drink, the patient's throat and mouth are well painted with the mixture, and he is directed to gargle his throat out with it. The results of pursuing this line of treatment have been, that in sixty cases of diphtheria he has only had one fatal case, though in Trieste the mortality from diphtheria is upwards of 350 annually, and he has had no secondary affections. (*Allgem. Wien. Med. Zeit.*, July 8, 1873.)

Dislocation of the Head of the Left Femur just above the Sciatic Notch.—An example of this rare form of dislocation is recorded in the new French journal, *Le Progrès Médical*, as having occurred at the Hôtel-Dieu, under the care of M. Richet. The patient was an active but heavy man, who, whilst turning round a group of people he wished to pass, with one foot on the pavement, was struck sharply on the back by a passing vehicle. He fell, and on attempting to rise found himself unable to walk; he was then carried to the house of a neighbouring surgeon, who told him he had a contusion of the hip, and ordered rest and cooling lotion. After the lapse of a month the dislocation was discovered, and a futile attempt at reduction made. He was then taken to the Hôtel-Dieu. The limb was found to be shortened to the extent of five centimetres, and there was well-marked rotation inwards. The patella being applied to the right popliteal space, the heel was depressed, and its posterior extremity was turned outwards. The great trochanter was much elevated and approximated to the iliac spine. There was a hollow corresponding to the cotyloid cavity, and the head of the bone could be felt moving within the trochanter when the leg was rotated in the external and posterior part of the iliac fossa. The gluteal fold was obliterated; the movements of adduction and abduction were found to be impracticable, though limited movements of flexion and extension could still be made. There was no pain, nor any numbness of the limb, which always accompany dislocation into the sciatic notch, owing to pressure on the sciatic nerve. M. Richet then proceeded to explain why the dislocation was not discovered in the first instance, and gave it as his opinion that it was secondary, and that there may have been in the first instance a fracture of the margin of the cotyloid cavity; and he related a remarkable instance of this where a young man, separating his legs widely, experienced sudden and violent pain, and was found by himself and Dr. Dumarquay to have the symptoms of fracture near the head of the bone, and was put up accordingly in a proper apparatus. To their astonishment, a few days after, the symptoms of dislocation of the hip joint appeared. They thought they had made an error in diagnosis, and speedily effected reduction by extension: but the dislocation recurred; was reduced a second time, recurred again; and then the head of the bone remained permanently in the external iliac fossa. Dr. Richet describes a second and similar case that occurred to him and Dr. Desruelles. These are both of great importance in a medico-legal point of view. M. Richet then proceeded to describe the apparatus required for reduction in detail, and pointed out how likely it was that all efforts might fail, especially when it was remembered that the dislocation had been of thirty-five days' duration. However, on the application of extension to

an amount not exceeding 250 lbs., as measured by the dynamometer, reduction took place, and a successful result was achieved. (*Le Progrès Médical*, No. 5, 1873.)

Treatment of pernicious Malarial Fever.—In the *Southern Medical Record* for May 1873, Dr. Benjamin Rhett, of South Carolina, has a paper upon Congestive Malarial Fever, in which he says: "In most cases of fever, intermittent or remittent, twenty grains of quinia, administered during the intermission or remission, are sufficient to arrest the next paroxysm. But the same quantity, twenty grains, administered during a paroxysm, has no control over that paroxysm, either during the cold or hot stage, but exercises its influence, if it has any effect at all, on the succeeding paroxysm. Indeed, the febrile symptoms are aggravated by giving quinia during the paroxysm.

"Suppose, now, we administer during the paroxysm, either during the cold or hot stage: \mathcal{R} Morphia, gr. $\frac{1}{4}$; chloroform, a teaspoonful; camphor, gr. ij, and repeat if necessary—the effect of the remedy is not only to *abort the chill*, but to *shorten the febrile paroxysm* by several hours. The same remedy administered previous to the hour of chill will sometimes postpone the attack several hours, and sometimes prevent it altogether."

He also reports several cases, of one of which he writes as follows:—

"I arrived about five o'clock in the afternoon, and found him in the following condition. His surface pale, with cool extremities and cold nose; features pinched, and finger-nails blue; the pupils of his eyes contracted to the size of a pin's head, and little sensible to variations of light. Soon the surface grew warmer, but the face, especially the nose and lips, became purple with venous congestion; the surface was soon bathed in profuse perspiration; the skin of the hands and feet became shrivelled, the nails being of a purple colour—the semi-comatose condition continuing. His breathing was embarrassed and stertorous, and there was considerable difficulty in swallowing. When partially roused to take medicine, his face had a wild, stupid glare, and he would relapse again into stupor with the spoon in his mouth. Having neither chloroform nor camphor with me, I administered one-fourth of a grain of morphia and twenty drops of turpentine every hour, plunged his feet into warm water, placed sinapisms upon legs, thighs, and arms, and hot flannels, with turpentine, around the abdomen, and gave turpentine enemata. When chloroform and camphor were obtained, he took, every two hours—morphia, gr. $\frac{1}{4}$; chloroform, a teaspoonful; camphor, gr. ij. This treatment was persevered in from 5 o'clock in the afternoon of the 13th July to 7 o'clock A.M. of the 14th, when there was a return to consciousness,—a subsidence of stupor.

A flow of urine, which had been suppressed, took place, and a subsidence of fever occurred. Quinia was given in full doses, and the patient soon rallied." (*Philadelphia Medical Times*, June 1873, and *South. Medical Record*, May 1873.)

A New Method for healing Ulcers.—Dr. Nussbaum claims to have successfully treated upwards of sixty cases of chronic, extensive, and otherwise intractable leg-ulcers, by the following simple procedure. It is at least worthy of a trial. The patient is first narcotised, and then around the ulcer of the leg or foot, a finger's breadth from its margin, an incision extending down to the fascia is made; numerous blood-vessels are divided, and a severe hæmorrhage ensues unless a fine pledget of lint be packed into the cut and the entire ulcer strongly compressed. The packing with lint is also necessary to prevent union of the cut edges by the following day. Upon the second day the bandage and lint are removed; from then until a cure is effected, a simple water-dressing is applied.

The author states that an astonishing change can be seen, even in the first twenty-four hours: the ulcer, which yesterday threw off quarts of thin, offensive, ichorous pus, furnishes to-day not more than a tablespoonful of thick, non-offensive, healthy pus. The old ulcer becomes rapidly smaller, healing from the margin towards the centre, and is healed in a short time, but the cut is changed into a broad circular sore, which also speedily cicatrises.

The great diminution of the secretion, and other favourable changes occurring in the ulcer, find an explanation from the fact that the circumcision has divided dozens of large, abnormally widened blood-vessels. Time is thus given for the lessened nutritive material, which previously was carried off by the excessive secretion, to be transformed into cells and connective tissue; in other words, granulations are formed, which fill up and heal the deep ulcer.

Without claiming this as a radical method, the author assures us that the cure is much more rapid, and the cicatrix becomes more elastic and resisting, than in the ordinary means applied, which usually require so much time that the patients depart with half-cured ulcers, soon to find themselves in their previous deplorable condition. (*Philadelphia Medical Times*, June 21, 1873.)

Treatment of Hæmorrhoids.—In the article on this subject in the just published part of the *Dictionnaire de Médecine*, M. de Lannelongue mentions the following plans of treatment that have been suggested:—1. Incision. This should be free, and after having been made the contents of the tumour should be squeezed out. Relapses often occur. 2. Extirpation. This may be total or partial, and may be accomplished with the bis-

toury, with scissors, with a ligature, or by means of the *écraseur*. When excision is practised with the bistoury or scissors, care should be taken to leave a little of the skin at the base of the tumour lest retraction of the anus follow, and also to prevent serious hæmorrhage. Removal by the *écraseur* is followed by little bleeding and no pain, but pyæmia is unfortunately not an unfrequent result. The rule laid down by Gosselin is an important one, namely, never remove simultaneously internal and external hæmorrhoids. The former disappear, or at least lead to no accidents, when the latter are cured. 3. Cauterisation. Three principal modes of effecting cauterisation have long been in use, namely, by the actual cautery, by solid, and by liquid caustics. Red-hot iron was applied as long ago as the time of Hippocrates. When adopted, it should be done thoroughly. Richet, combining *écrasement* with cauterisation, uses forceps, which, brought to a white heat, are made to constrict the base of each hæmorrhoidal tumour, leaving some space between each point of application in order to prevent retraction of the anus. Jobert de Lamballe has invented a peculiar kind of forceps, which embrace the tumour and permit the ready application of the cautery without any risk of injury being inflicted on the skin. The action of Vienna paste can thus be easily localised. Amongst the solid and liquid caustics, the caustic of Filhos and nitric acid are the best. (*Nouveau Dictionnaire de Médecine et de Chirurgie*, 1873, p. 431, tome xvii.)

Bread Tests and Adulterations.—Experience has everywhere shown (says a writer in the *Sanitarian*) that bread made from the cereal grains contains more of the essentials necessary to the support of adult human life than any other article of food. To the cereal grains may be added buckwheat, which, although belonging to a different family of plants, is nevertheless in its composition analogous to the cereal grains. Bread is made either by fermentation, or by forcing in carbonic acid, or by forming carbonic acid in the substance of dough which consists of flour mingled with water. In the manufacture of ordinary fermented bread, alum is added, ostensibly to prevent excessive fermentation, but this salt also possesses the quality of incorporating an excess of water and increasing the weight of the loaf. In making bread by this process the proportions are 20 lbs. of flour, 8 to 12 lbs. of warm water, 4 ozs. of yeast, a little mashed potato, and $1\frac{1}{2}$ to 2 ozs. of salt. $6\frac{1}{2}$ lbs. of dough should yield 6 lbs. of bread. Carbonic acid is also disengaged by mixing soda and cream of tartar with the dough, or by the use of baking powders, which are compounds of the salts of sodium and ammonium, and tartaric, phosphoric, and citric acids. Aërated bread is made by forcing carbonic acid through

the dough by means of pressure. A barrel of flour will make from 63 to 73 good 4-lb. loaves of bread. Good bread only contains 33 per cent. of water; all over this proportion is in excess. *Cæteris paribus*, the greater the amount of gluten in flour the more valuable it is, as it is easy of digestion and highly nutritious.

M. Boland and M. Robine have both suggested instruments by which the proportion of gluten present may be approximately estimated. The instrument invented by the former is termed the *Aleurometer*, and essentially consists of a cylinder in which a certain quantity of the gluten from a sample of flour is subjected to the heat of an oil bath at 420 degrees Fahr. The amount of swelling is taken as a test of the quality of the gluten.

The *Appréciateur* of M. Robine is based upon the property of dilute acetic acid of dissolving out the gluten or albuminous matter in flour, without affecting the other constituents. The density of the solution is the measure of the richness of the flour in gluten.

Variations in the quality of the gluten may be due to the admixture of corn-meal, rye flour, barley flour, oatmeal, pea-meal, bean-meal, buckwheat-meal, potato-starch, and rice flour. The chief adulterations are the various compounds of lime, as marble dust, plaster of Paris, chalk, bone-dust, alabaster dust, mineral white and gypsum, alum, pipeclay, soapstone, and sulphate of copper. (*Sanitarian*, No. 4, 1873.)

Heat in the Treatment of Paralysis.—Dr. Fisher observes that heat as a means of treatment in paralysis seems to have received very little attention from practitioners in general, and few authorities make more than a reference to its employment; and yet experience has shown, and will show to whosoever will seek it, that we possess in it one of our most serviceable aids. It is cheaply and easily applied; it is most agreeable to the patient, and its results entitle it to a far higher consideration than it now receives.

On *a priori* grounds we should be led to expect good results from the local application of heat in paralysis, from the benefit which we obtain from it in other instances of depressed vitality. Its salutary effects in shock after injuries, in pneumonia, rheumatism, gout, enteritis, etc., are well recognised. Larrey found, in the treatment of chronic ulcers by warm air alone—a method which he introduced from Egypt—that when they were kept, without interruption, at a uniform temperature of 86° Fahr., the reparative process was more active than under any other treatment whatsoever. The comparative ease and freedom with which a hemiplegic patient can flex and extend his fingers while

lying warm in bed, before rising in the morning, are in strong contrast with the stiffness of the hand after exposure to the cold air.

The effect of warmth locally applied is to produce an expansion and relaxation of tissue, with temporary engorgement of the blood-vessels of the part; and, as a consequence, the interchange between the nutrient fluid and the tissue-cells is promoted, and nutrition is more or less improved. All the modes of applying heat accomplish these ends to a greater or less extent, and the question of employing it in the form of hot water, vapour, or air, should be decided by considerations of convenience and the objects of treatment.

The use of dry heated air is to be preferred in the treatment of paralysis for the following reasons:—It is easy of application; it can be carried to a much higher degree of temperature; can be applied oftener and continued longer than water or vapour; and the heat is retained for a much longer period by the limb to which it has been imparted. When hot water is employed, the sense of touch and the muscular contractility are always blunted for a time, the skin is softened, and the subsequent evaporation from its surface renders the patient much more liable to take cold than after the dry method.

Plunging the limb into hot water and then into cold has been recommended as a local stimulant to sluggish circulation. The local use of vapour for the purpose under consideration is seldom resorted to. It combines the disadvantages of hot water with others which are peculiar to itself. Vapour is precipitated upon the surface, evaporation does not take place, and perspiration is retarded. The quantity of moisture, therefore, upon the skin does not necessarily measure the sudoriparous qualities of the particular agent which may be employed; for when dry hot air is used, the perspiration is taken up rapidly, and does not show itself in drops until it becomes secreted in such quantity that evaporation cannot carry it off.

Dr. Conrade, of London, has recently revived the use of sand as a vehicle for the application of dry heat to the body. He gives a good description of the sand-bath and its effects in health and disease in the *British Medical Journal* for Nov. 3, 1872. It is an excellent contrivance for acquiring comparatively low temperatures (100–110° Fahr.), which, in certain conditions of disease, are preferable to the higher degrees which the Turkish bath affords; but for the local treatment of paralysed limbs it is not as serviceable as hot air.

The necessary apparatus for the employment of local heat as a means of treatment is simple enough and within the reach of everyone. All that is needful is an alcohol lamp, or other similar means for generating heat, and a contrivance for retaining

it about the paralysed limb. Dr. Fisher uses a box, containing a gas stove, with a heavy curtain in front, which is arranged in such a manner that the patient can be seated comfortably before it in a chair, with an arm or leg reclining within on a non-conducting support. The gas is lighted, and the patient is at liberty to read or amuse himself in any other way until the limb is thoroughly heated. This may require from a quarter to half an hour; but there is no other rule for regulating the length of the application or the degree of temperature. When the warming has been complete, its good effects are shown, not only in the redness of the skin and the increased circulation, but the patient experiences a grateful sense of power in the part, so that when voluntary motility exists to any degree, muscular movements can always be executed with more force and less effort than before the heat was applied. Electro-muscular contractility is likewise much improved by the same means. The application of heat is, therefore, a serviceable preliminary to electrification and the use of active and passive exercises.

Warmth is of value in all forms of palsy, and at all periods of its continuance. In the early stages of the severe varieties in which the paralytic is confined to bed, it may be more convenient to use heated bricks, or bottles filled with hot water, than the methods which have been indicated above. But, however applied, the heat not only adds greatly to the comfort of the patient, but acts as a positive conservator of power against the retrograde changes in the extremities which follow the central lesion. Flannel and woollen garments next to the skin are serviceable at all times in maintaining the natural warmth. In general terms, the less perfect the circulation, and the colder the limb under ordinary circumstances, the more marked are the effects from artificial heat; but the benefit that may be expected from it in any case must be determined by the nature of the paralysing lesion, the extent of its results, and the general condition of the patient.

The localised hot-air bath, therefore, is a valuable means of treatment, which may be left in the hands of a patient, or some one of his family, with perfect safety. The opportunity of being able to afford a case of paralysis the chance to "do something," during a course of treatment which is often necessarily slow and tedious, is not without its advantages, the more especially when that something is really of decided benefit. (*Brown-Séquard's Archives of Scientific and Practical Medicine*, No. 4, 1873.)

Faradisation in lieu of Ergot in the last Stage of Delivery.—The discussions which have recently taken place in regard to ergot have by no means shaken the confidence of the

public or of the profession in its value, but have simply served to determine more precisely the conditions under which it should be used. M. L. Championnière trusts that practitioners will not regard ergot as a means of hastening labour alone, but that they will employ it when they have satisfied themselves of the dilated condition of the os uteri, and as a means for awakening uterine action in cases of absolute or relative inertia. He approves of the suggestions that have lately been made by M. Tripier for the employment of faradisation in the later stages of delivery. Whenever labour has fairly commenced, and the pains succeed regularly with an interval of about a quarter of an hour, M. Tripier, following the example of M. de Saint-Germain, faradises the lumbar region by two excitors. In a very short space of time, fresh activity becomes apparent in the uterine contractions, and the pains recur more frequently; the uterine contractions become longer and more painful, and the dilatation of the neck is observed to take place with great rapidity. In all the cases in which it was tried, the expulsion of the placenta took place immediately after that of the foetus, but in no instance did the child appear to have suffered any harm, although by its vigorous movements it clearly showed that it perceived and responded to the excitation of the currents. M. de Saint-Germain passed the current for twenty minutes at a time, but M. Tripier finds this rather too long, and only uses it for from five to ten minutes. For several years past M. Tripier has never attended a confinement without using such currents, passing them from the sacrum to the sub-pubic region, or even from the sacrum to the uterus itself, and all have borne it well. The delivery has been in all instances rapid, and there have never been any ill consequences. The mode of application consists in placing the positive pole on the lumbar region, and the negative immediately below the pubis. Where severe hæmorrhage has been present, the negative pole, represented by a good-sized olive, is introduced directly into the interior of the uterus. (*Journal de Médecine*, July 1873.)

Notes and Queries.

CORRESPONDENCE.

HOMŒOPATHIC PILULES.—In reply to a letter from Messrs. Capper, of Liverpool, homœopathic chemists, who complain of our analysis of their pilules (or rather of the inferences drawn by us from that analysis), we have merely the same answer to make as to a similar complaint which was noticed in our August number. We never had the slightest reason to doubt the integrity of the chemical manufacturers in question, and are glad to find that, even in their first dilution pilules, homœopathic chemists do not pretend to put doses appreciable by chemistry. That simplifies matters greatly!

Department of Public Health.

THE MEDICAL OFFICER OF HEALTH.—HIS ADMINISTRATIVE AND CLERICAL DUTIES.

THE regulations of the Local Government Board as to the administrative and clerical duties of the Medical Officer of Health form one of the most complex of the many puzzles which the Board has originated in its initiation of the Public Health Act, 1872. He is to attend at the office of the Sanitary Authority, or at some other appointed place, at such stated times as they may direct. He is from time to time to report, in writing, to the Sanitary Authority his proceedings, and the measures which require to be adopted for the improvement or protection of the public health in the district. He is in like manner to report with respect to the sickness and mortality within the district, so far as he has been enabled to ascertain the same. He is to keep also a book, or books, to be provided by the Sanitary Authority, in which he shall make an entry of his visits, and notes of his observations and instructions thereon, and also the date and nature of applications made to him, the date and result of the action taken thereon, and of any action taken on previous reports, and shall produce such book or books, whenever required, to the Sanitary Authority. He is further to prepare an annual report, to be made to the end of December in each year, comprising tabular statements of the sickness and mortality within his district, classified according to ages and localities, and a summary of the action taken during the year for preventing the spread of the disease. The report shall also contain an account of the proceedings in which he has taken a part or advised under the Sanitary Acts, so far as such proceedings

relate to conditions dangerous or injurious to health, and also an account of the supervision exercised by him, or on his advice, for sanitary purposes, over places and houses that the Sanitary Authority has power to regulate, with the nature and results of any proceedings which may have been so required and taken in respect of the same during the year. He is also to record the action taken by him, or on his advice, during the year, in regard to offensive trades, bakehouses, and workshops. Finally, he is to give immediate information to the Local Government Board of any outbreak of dangerous epidemic disease within the district, and to transmit to the Board, on forms to be supplied by them, a quarterly return of the sickness and deaths within the district, and also a copy of each annual and of any special report.

The different administrative and clerical duties here set forth, notwithstanding the apparent complexity of the latter duties, present few, if any, difficulties of execution in districts where there is but a single Sanitary Authority with whom the Medical Officer of Health has to deal. In such districts there is abundant experience to guide the newly appointed Medical Officer of Health in the exercise of his duties. He will do well, indeed, whether his district be rural or urban, to make himself acquainted by personal observation with the administrative and clerical working of some of the districts in which Medical Officers of Health have been long established; as, for instance, the borough of Liverpool, under the supervision of Dr. Trench, and the metropolitan districts of Whitechapel and Marylebone, under the supervision of Mr. John Liddle and Dr. Whitmore respectively. Such personal observation is all the more needed, as the Local Government Board has made no suggestion and gives no instructions as to the forms of the different returns which the Medical Officer is called upon to make. It was much to be desired that Sanitary Authorities and their Medical Officers should, by suggestion, have received some assistance in devising the different returns required in the Sanitary administration of a district. It was equally to be desired, for the purpose of facilitating comparison, that the returns of different districts should be as far as practicable prepared upon a uniform plan. The Local Government Board has, however, thought fit to leave

Sanitary Authorities and their Medical Officers to act in this respect according to their own devices, thus fostering a discordancy in the returns and reports of different districts absolutely fatal to that general utilisation of the work of the Medical Officers of Health in the interest of the public as a whole which the Royal Sanitary Commission contemplated. Even the form for the quarterly returns of sickness and death to be made by the Medical Officer of Health to the Local Government Board, and which it is to be presumed would yield useful hints for his returns on the same subjects to his Sanitary Authority, has not been issued.

But although the regulations as to the administrative and clerical duties of the Medical Officer of Health present little, if any, difficulty in their application to districts having only a single Sanitary Authority, when they are considered in their application to districts formed by the combination of several Sanitary Authorities we are plunged into absolute bewilderment. In these combination districts the regulations as to the duties of the Medical Officer of Health are presumed to apply independently to *each* district entering into the combination. The Medical Officer of Health, in fact, is understood to undertake to carry out in each of the several combined districts the duties set forth in the Regulations. The Banbury and Abingdon combined districts each include thirteen urban and rural sanitary districts; the Barkisland combined district includes seventeen urban and rural sanitary districts. To each of the combined districts *one* Medical Officer of Health has been appointed. Read the regulations as to appointed place of attendance and as to duties of returns and reporting—read, indeed, the regulations as to duties generally, and ask how it would be possible for one man to carry them out in the several districts entering into the combination to which he is appointed. The notion that he could do so is simply preposterous. Yet, under the direct instigation of the Local Government Board, combination districts have been formed, Medical Officers of Health have been appointed to them, and the Sanitary Authorities advised that the duties of the Medical Officers of Health will be such as are set forth in the Board's regulations, although these regulations are for the most part wholly impracticable under the circumstances.

Parliament will, no doubt, in due time resolve this amazing circumlocutional puzzle, but meanwhile the mischief done by these freaks of organisation is immeasurable. Thorough sanitary work under such an organisation is impossible, and both the central and the local sanitary administration are discredited in the estimation of the public.

The difficulties experienced in these combination districts of making those collateral arrangements which are necessary for the proper exercise of the Medical Officer of Health's duties have proved all but insuperable to the present moment. We believe that we are accurate in stating that in some of the most important of them the Medical Officer of Health does not receive any returns, either of mortality or sickness, for his several districts, and that he is practically dependent for information in these fundamental matters upon material incidentally gleaned by the inspectors of nuisances. But if it should so happen, that ultimately arrangements are successfully made for the Medical Officer of Health being furnished weekly by each of his several authorities with returns of deaths, of pauper sickness, and of sickness in public institutions, and that he be placed in a position to carry out, in respect to his authorities, all the regulations of the Local Government Board, as to bookkeeping, reporting, and correspondence, he will either have to be furnished with a staff of clerks, or will have to restrict himself entirely to his clerical duties.

The gentlemen who have been appointed Medical Officers of Health for combined districts are doing the best they can under the difficult circumstances in which they are placed, but that best is only practicable by holding the regulations issued for their guidance for the most part a dead letter.

THE SANITARY STATE OF THE STAFFORDSHIRE POTTERY DISTRICTS.

A SERIES of elaborate official Reports on the sanitary state of the Staffordshire Pottery district, from the pen of Dr. Edward Ballard, has recently been published by the Local Government Board. The following is a summary of these Reports:—

The Pottery district of Staffordshire is situated at the extreme northern part of the county, and is included within two registration districts,—namely, that of Stoke-upon-Trent and that of Wolstanton. The manufacture of pottery, however, is not carried on over all parts of these districts, but is confined to certain towns with their suburbs. These towns are Stoke-upon-Trent, Longton, Fenton, Hanley-with-Skelton, Burslem, and Tunstall. In other parts of the district, iron-mining, forging, and coal-mining are carried on. Some of the villages in the neighbourhood of these latter industries have of late years grown rapidly, and have assumed the appearance of small towns. Such as these are Silverdale, Chesterton, and Kidsgrove. Hanley (including Skelton) and Longton are corporate towns, the local sanitary authority in which are the mayor and corporation; Stoke and Fenton are severally governed by a self-elected body of Improvement Commissioners; while Burslem and Tunstall, and recently Kidsgrove, have each a Local Board as the sanitary authority. The remainder of the two registration districts mentioned is now, for all sanitary purposes, under the jurisdiction of the guardians of the parish of Stoke-upon-Trent, or of the union of Wolstanton and Burslem. Up to the passing of the Public Health Act of last session, the duties now devolving upon the guardians were divided between them and the vestries of the several parishes of Stoke, Wolstanton, and Burslem; the guardians being the authority for carrying out the Nuisances Removal Acts, while all matters relating to drainage, water supply, and the removal of excrement and refuse, fell within the jurisdiction of the vestries. This division of responsibility was existent at the time when the inspection was made, and much of the delay which has taken place in the issue of the reports has been due to the necessity of adapting the recommendations for amendment to the new conditions of sanitary administration.

Each sub-district is made the subject of a separate report. Several advantages have been thus gained—one of the principal of which has been that the mortality in the several parts of the Potteries, as ascertainable from the death-registers, could be placed side by side with the observed conditions under which the population of each sub-district lived; and comparisons could be made and inferences drawn which otherwise would have

been impossible. The principal disadvantage of this method of reporting is an apparent want of cohesion; it has, however, been reduced to a minimum by the adoption of one uniform plan throughout the reports. And inasmuch as sometimes several authorities were found to have jurisdiction within a single sub-district, the portion assigned to each authority is separately treated of in the report upon that sub-district.

The subjects brought under notice in each report are thus arranged:—Locality, site, soil, population, industries, death-rate, both general and from certain specified diseases, sewerage and drainage, paving and scavenging, water supply, the mode in which excrement and refuse are dealt with, the mode in which nuisances are dealt with, the condition of the dwellings of the poor, and, lastly, the mode in which sanitary administration is carried on. Each authority is there presented with a full series of recommendations for the amendment of the district under its control, and for the improvement of its sanitary administration.

This being so, it becomes practicable to make the following summary of the Reports:—

Site.—With the exception of Stoke-upon-Trent, the greater part of which is built upon the nearly level land of the Trent valley, all the pottery towns occupy elevated sites on the summits and sides of hills, the suburbs of one town joining on to those of another by scattered dwellings across the intervening valleys. The largest of the pottery towns is Hanley, the area of the borough being 1,790 acres. The summit of the hill upon which it stands is 643 feet above Ordnance datum. The borough of Longton extends over 968 acres, and the highest part of the town has an elevation of 567 feet. The acreage of the Local Board district of Burslem is 1,600 acres, and the highest part of the town stands at an elevation of 515 feet. Tunstall is not quite so high, and the extent of the district of the Local Board is only 690 acres. All these elevated towns, as well as the village of Fenton, which unites Stoke and Longton, are very well situated for drainage, and also for the free passage of air through and across their streets. Most of the mining villages about the district also occupy elevated sites.

Soil.—In the lower parts of the district the soil beneath the surface earth consists of gravel, clay, or brick earth; the hills

are of sandstone overlying ironstone and coal. Mining operations have extended beneath some of the towns.

Population.—In 1871 the population of the two registration districts of Stoke and Wolstanton was 158,156. At the previous census in 1861 it was 125,664. The increase of population has not been confined to the pottery towns; indeed, the greatest increase occurred in the sub-district of Wolstanton, which includes a mining district in which large iron-works have also been established. The population of the several pottery towns in 1871 was as follows:—The borough of Hanley (which includes Skelton), 39,976; the Commissioners' district of Fenton, 10,556; the borough of Longton, 19,748; the Commissioners' district of Stoke, 14,008; the Local Board district of Burslem, 25,562; the Local Board district of Tunstall, 13,539. The population of the larger mining and iron-workers' villages in 1871 was as follows:—Kidsgrove, 803; Chesterton, 3,783; and Knutton township, the most populous part of which is the village of Silverdale, 6,386.

Industries.—The manufacture of the various kinds of pottery, common earthenware, stoneware, china, encaustic tiles, &c., is confined to Hanley, Longton, Fenton, Stoke, Burslem, and Tunstall, with their suburbs. There are also several large brick-making works in places where a suitable clay is obtainable. The principal iron-works are those of Earl Granville, close to Hanley, and those at Silverdale and Kidsgrove; but there are others at Tunstall and elsewhere. Enormous mounds of slag and refuse have been accumulated where these iron-works have been established, and heaps of ironstone undergoing calcination in the open air pollute the atmosphere with sulphurous acid. The addition of large volumes of smoke from the iron furnaces and pottery ovens renders the atmosphere of the pottery towns constantly murky. The effect of the pottery industry upon health and mortality is contained in an appendix to the Reports.

Death-rate.—The average annual general death-rate from all causes and at all ages, per 100,000 living, during the ten years 1851-60, in the registration district of Wolstanton, which includes Tunstall and Burslem, was 2,617; and in that of Stoke, which includes Hanley and Skelton, Stoke, Fenton, and Longton, was

2,621. During the ten years 1861-70, it was, for the registration district of Wolstanton, 2,549, and for that of Stoke, 2,546; so that on the whole some improvement appears to have taken place. But when compared with the average general death-rate of England and Wales, which during the ten years 1851-60 was 2,217, it is much too great. The death-rate has been highest in Longton, where the average during 1861-70 reached 3,064. The other sub-districts follow in order, thus:—Fenton, 2,664; Burslem, 2,617; Tunstall, 2,580; Skelton (in the borough of Hanley), 2,381; Wolstanton (a mixed iron-working, mining, and agricultural sub-district), 2,338; Stoke, 2,331; and Hanley, 2,308. The average annual death-rate of infants under one year of age varied from 28,829 per 100,000 of such infants living in Longton, to 18,612 in Stoke. It was as high as 25,180 in Fenton, and 24,655 in Tunstall. A table in the appendix to the Reports shows the death-rates from “fevers,” bowel diseases, scarlet fever, and diphtheria, at all ages; from bowel diseases, diseases of the respiratory organs, diseases of the brain, small-pox, scarlet fever, measles, and whooping-cough, at ages less than five years; from phthisis and other diseases of the lungs, at working ages—that is, at ages between 15 and 55 years; and lastly, from diseases of the brain, at ages between 35 and 55 years. Other tables show in respect of the two sexes separately the average annual death-rate from phthisis and other diseases of the lungs at different periods of working age. The average annual mortality from fever was highest in Longton—viz. 130 per 100,000 living—and in the other sub-districts varied from 92 in Tunstall to 65 in the sub-district of Wolstanton; that from bowel disorders was highest in Burslem—viz. 201—and in the other sub-districts varied from 169 in Longton to 119 in Wolstanton; that from scarlet fever was highest in Burslem—viz. 135; and in the other sub-districts varied from 128 in Longton to 81 in Hanley; that from diphtheria was highest in the open, straggling, sub-district of Fenton—viz. 46; and in the other sub-districts varied from 29 and 28 in Hanley and Skelton respectively, to 8 in Longton. During the recent epidemic of small-pox, Burslem, Hanley, and Skelton suffered least, but Longton was visited very heavily, no fewer than 106 deaths from this disease having been registered during the second and third quarters of 1871. Whenever

scarlet fever or measles have broken out in an epidemic form, they have also been exceptionally fatal, especially in the towns of Longton and Burslem.

Sewerage and Drainage.—The only pottery town which can lay claim to a fairly complete system of drainage is Stoke, and this only during recent years. Fenton is quite underground, and so also is Longton, with the exception of one small part. In both these places sewage flows along the surface wherever it can find a free course, and it lodges wherever it can find a lodgment, ultimately soaking into the earth. Some of it, however, finds its way into some imperfect superficial drains, which carry it into watercourses that empty themselves at length into the Trent. Even the large town of Hanley is very imperfectly sewered: some of the old watercourses, which form the main sewers of the town, have been covered in, while others have been left open to be constant sources of nuisance; the subsidiary sewers, where they exist, are imperfect and inefficient, while some parts of the town are totally devoid of any means of drainage except the open street-channels or courses which slops and domestic sewage thrown out into the roadways have made for themselves. Burslem is generally provided with main sewers, newly constructed; but there still remain some open sewers which are offensive and dangerous to health, and there are parts of the town where domestic sewage is still thrown into the roadway, either to soak into the earth or to run along the surface until it reaches a grid in the highway. Defects of detail in drainage here serve to lessen the good which the main sewerage of the town ought to have effected for it. Much the same may be said of Tunstall, part of which has been newly sewered, while the rest is provided for by an old and very imperfect system of drains about which nobody knows very much. Since the beginning of 1870 the Local Board has been hampered in its endeavours to provide proper drainage, by an injunction restraining them from making any new communications with their main sewers, until means have been found for disposing harmlessly of their sewage. It may therefore be said that, with the exception of Stoke, nuisances arising from the absence of any drainage, from imperfect sewerage and drainage, or from defects in the most important details of drainage, abound in

such a degree as to endanger health in all the pottery towns. The village of Kidsgrove is, in respect of drainage, much in the same condition as Tunstall. Neither here nor in any of the towns mentioned is there any plan in operation for disposing harmlessly of the sewage, all of which that does not soak into the ground finding its way into watercourses, and finally into the Trent. Some partial and very imperfect attempts at utilising the sewage by irrigation do not materially affect the correctness of this statement. No proper system of main sewerage and house drainage is found in operation in the other large villages; imperfect attempts at drainage have been made, but they altogether fail to effect their object. And with regard to the smaller villages scattered about the more rural parts of the sub-districts of Stoke and Wolstanton, it may be said that there are no means of drainage whatever. Offensive sewage runs along the sides of the roadways, and lodges in disgusting pools anywhere; or, if it has a sufficiently long course, soaks gradually into the earth, or gradually evaporates.

Water Supply.—The pottery towns and also the villages of Wolstanton, Goldenhill, and Kidsgrove, together with some outlying villages near Stoke, are supplied with water by the Staffordshire Potteries Waterworks Company. The quantity supplied in several parts is however very inadequate, and some places have at times been altogether without water for many days together. The Company appears to have undertaken more than it can well perform. Even in Hanley the upper parts can only be supplied when the water is shut off from the rest of the district. The water is not filtered, and consequently is sometimes supplied in a condition unfit for drinking. In parts of the registration districts of Wolstanton and Stoke not supplied by the Company, the people have recourse to public or private wells, most of which furnish undrinkable water; to springs, many of which are polluted, some very dangerously so, with domestic or town sewage, and liquids oozing from privy tanks; or to land drainage water, or the water pumped out of the mines. In one instance a record is made of two villages which derive their water supply mainly from some "spouts," one of which drew water directly from a field drain which received the overflow of a sewage deposit pond; and in another instance an out-

break of enteric fever is traced to the use of the water of a spring polluted by receiving the sewage of a number of houses which, after skirting an imperfectly constructed and full privy tank, flowed directly into the spring. Full details are given of nearly every spring and pump in these districts supplying drinking water to the population.

Disposal of Excrement and Domestic Refuse.—Water-closets are, as might be almost assumed from what has just preceded, exceptional conveniences throughout the Potteries. The usual system adopted is to store excrement in brick tanks attached to the privies. These privies, with their tanks or cisterns, are everywhere abominable nuisances; they were commonly found over-full, especially in Longton and Fenton and in the villages, often leaking, and sometimes leaking into dwelling-houses. In some places where the tanks are altogether sunk into the ground the covers were found broken in, the contents of the tanks being thus exposed, and on the occurrence of rain flooding the adjoining surface of the ground. Throughout the Potteries also, even in such a large town as Hanley, ashes and domestic refuse choke up the ashpits with large accumulations, or are deposited, without any interference of the Local Authority, in the roadways or on any waste land that may be convenient for the purpose. All this not only endangers health in various ways, but gives to the pottery towns and villages an aspect of universal neglect.

Dwellings of the Poor.—The dwellings of the poor and artisan classes quite correspond with the conditions of bad drainage, defective water supply, and excrement nuisances among which they are placed. In the towns, with the exception of Stoke and generally of Tunstall, they are unwholesome, crowded together in narrow, close, unpaved, and undrained streets or courts, often dirty, and more or less dilapidated. As a rule, the poor and artisan classes in the Potteries have an almost insuperable dread of fresh air in their cottages. Hence the practice, almost universal among them, of bricking or pasting up the flues in the bedrooms, and that of so constructing the windows that they cannot be opened at all only to the extent of one small pane of glass. The detailed description given of some of the worst of these cottages, which are designated as quite unfit for the habitation of a human being, shows how entirely, in some parts, the

Local Authorities have neglected to perform their most obvious sanitary functions. Towns and villages are all alike in this respect. Town Councils, Commissioners, Local Boards of Health, and Guardians, all fall within the same condemnation.

Of all the pottery towns, Longton, however, is pointed out as the closest, worst ventilated, worst drained, and generally, as respects its surface, its dwellings and its atmosphere, the dirtiest and most polluted. Few people are said to reside there who can afford to reside out of it. All this quite corresponds with what has previously been stated as to its death-rates.

ENTERIC FEVER FROM INFECTED MILK.—THE OUTBREAK IN THE METROPOLIS.

THE parishes of Marylebone and St. George's, Hanover Square, are at the present moment the scene of an outbreak of enteric fever remarkable in its history and limitation. The details of the outbreak are only yet imperfectly known, and it is feared that when they are fully ascertained the number of cases which have occurred will probably amount to several hundreds. The families attacked belong with hardly an exception to the wealthier classes, and among them are those of numerous well-known physicians and surgeons. The disease has exhibited its greatest violence in the streets adjoining Cavendish Square, Portman Square, and Grosvenor Square, but scattered cases have occurred over the whole area of the parish of St. Marylebone, and over the northern portion of the parish of St. George's Hanover Square. The outbreak began in the third week of July, and among the families earliest attacked was that of Dr. Murchison, five of whose children, we regret to state, now lie ill of the disease. The children were attacked in two groups, so to speak, at an interval of eight days, and circumstances connected with the second group of cases led Dr. Murchison to suspect that the probable source of infection was the milk used in the family. While this suspicion was haunting his mind, numerous cases of enteric fever came to his knowledge in the vicinity of Wimpole Street, all occurring in families who obtained

milk from the same source as that used in his own family. Dr. Murchison, now fully alarmed, cast about for further information, and very shortly he obtained the particulars of enteric fever then prevailing in about thirty families living for the most part in the vicinity of Cavendish Square, Portman Square, and Grosvenor Square. In these families the disease had appeared almost simultaneously, under conditions which did not admit of explanation from defects of sewerage, drainage, or water supply; in many of them it had attacked several members about the same time; young children formed a considerable proportion of the persons attacked; and there apparently was but one known condition common to the whole of the families, with the exception of two or three, by which the infection of enteric fever could have been conveyed,—namely, the milk supply. All the families, with the exceptions referred to, obtained their milk from the same dairy.

Dr. Murchison's suspicion now appearing to be largely confirmed, he communicated with the Medical Officer of Health for Marylebone, Dr. Whitmore, and measures were immediately adopted to stop the issue of milk from the suspected dairy. The directors of the dairy, incredulous as to the infection of their milk, at first hesitated to take any measures for stopping its issue or for seeking the source of infection, and it was not until the aid of the Medical Department of the Local Government Board had been sought, and after several days had been lost, that active steps were taken to search for the source of mischief. Pending this action additional facts came to the knowledge of Dr. Murchison, which more directly implicated the milk supplied from the dairy as being the source of mischief. Two of four servants left in charge of a house, on board wages, were seized with enteric fever. The servants attacked used milk from the suspected dairy; the servants who escaped the disease used preserved Swiss milk. Enteric fever appeared in one of two adjoining houses, both having milk from the suspected dairy, and both receiving their milk from the same delivery can. In the family which escaped, the milk was invariably scalded, but among the persons using the scalded milk there were several cases of obscure indisposition. The family which was infected used the milk in the state in which

it was received. A lady who had been on a visit in London, and who during her stay in town used milk largely from the suspected dairy, was seized with enteric fever in three or four days after returning into the country. No other person among a large family, including several young children, suffered from the disease. A young girl spent the day with a family which had its milk from the suspected dairy, and during the day she drank about two pints of the milk: four days after she became indisposed, her sickness eventually proving to be enteric fever. About the same time eight members of the family she visited, all using milk from the suspected dairy, sickened of the disease. In many streets enteric fever appeared only in families having milk from the suspected dairy; in not a few families those members only were attacked who used the milk.

A medical inspection, conducted by Mr. J. Netten Radcliffe for the Medical Department of the Local Government Board, Dr. Whitmore for the Vestry of Marylebone, and Dr. Corfield for the directors of the suspected dairy—these gentlemen being accompanied also by Mr. Chalmers Morton on the part of the dairy company,—was now made of the farms from which the dairy obtained its supplies of milk. These farms were eight in number, two of them situated in the vicinity of Oxford, the remainder in the neighbourhood of Thame. The inspection showed that no suspicion as to the issue of infected milk attached to seven of the farms; but as to the eighth, there was a strong probability, amounting for practical purposes to a certainty, that infected milk had been issued from it at a time immediately preceding and during the development of the outbreak of enteric fever in London. The farm itself was situated in a district in which enteric fever had been prevalent for several months, and all its labour was obtained from villages infected with the disease. The occupier had died from heart-disease (as certified) while recovering from, and in the fifth week of enteric fever, on the 8th June. A son was ill of enteric fever at the time of inspection. Several modes could be suggested in which the milk might have become infected, enteric fever being on the premises; the most probable mode, however, was through the water used for dairy purposes. This was manifestly impure, and it was exposed to excremental and

other pollution. Analysis of specimens taken at the time of the inspection proved that it was charged with matter in an active state of putrefaction, and that it was polluted with sewage.

Upon this evidence all issue of milk from the farm was at once suspended.

Such is a brief history of this outbreak to the present time. We have detailed the circumstances of the outbreak simply as they bear upon its past development in July, but there is every reason to believe that scattered cases of enteric fever, which occurred before July in the district of the outbreak, owed their origin to infected milk. A detailed official investigation of the outbreak is now being carried out by the Medical Department of the Local Government Board; and it is understood that this investigation will extend to the conditions of the metropolitan milk supply, with special reference to the modes in which the milk may become infected with the poison of enteric fever.

THE ARTIFICIAL FRUIT ESSENCES.

DR. HENRY K. OLIVER, in a report on the character of substances used for flavouring articles of food and drink, prepared for the State Board of Health of Massachusetts (see *Fourth Annual Report* of the Board), gives much interesting information respecting the use of artificial fruit essences. The compound ethers which have been found to possess the odour and flavour of certain fruits are several in number.

Butyric Ether.—This ether may be prepared by mixing butyric acid with sulphuric acid and alcohol. The former acid may be made by mixing decaying cheese with grape-sugar and chalk, and allowing fermentation to take place. The ether is dissolved in another portion of alcohol, and forms the pine-apple essence.

Pelargonic Ether, Oxuanthic Ether, may be prepared by digesting pelargonic acid with alcohol at a gentle heat. The acid is

the result of the action of nitric acid on oil of rue. The ether is dissolved in alcohol, and forms the quince essence.

Acetate of Amylic Ether is prepared by distilling a mixture of fusel oil, acetate of potash, and concentrated sulphuric acid. An alcoholic solution of the ether forms the jargonelle pear essence.

Valerianate of Amylic Ether may be made by the action of sulphuric acid and valerianic acid upon fusel oil. An alcoholic solution of this ether forms the apple essence.

A mixture of acetate of amylic ether with butyric ether forms the banana essence.

Other mixtures of the ethers, modified by the addition of various agents, as nitrous ether, acetic ether, acetic acid, camphor, tincture of orris, vanilla, the volatile oils, &c., result in imitations of other fruits, the strawberry, raspberry, apricot, currant, &c. &c.

Are these artificial fruit essences deleterious to health? To this question, says Dr. Oliver, a succinct answer cannot properly be given. If taken into the stomach in any considerable quantity and in an undiluted form, the effect would, without doubt, be not simply deleterious, but highly dangerous. But in the form in which they are presented in confectionery, &c., they are more or less diluted; the chance, therefore, of harm following their occasional use is greatly lessened. But when diluted, habitual indulgence in them, according to the opinion of scientific men, cannot fail to be injurious to health; and deleterious results may follow their occasional use, even when in the diluted form: this may happen in the case of adults, on account of a peculiar idiosyncrasy. And in all cases, probably, Dr. Oliver thinks, children are more susceptible to their influence than adults. Children are also more likely than adults to partake largely of confectionery, and a free indulgence in articles of this kind, in a season of the year when diseases of the intestinal canal are prevalent, has been known to bring on such disorders, or to augment them where existing. It is, however, sometimes the case that sufficient dilution has not been made, and alarming consequences have attended such carelessness. Dr. Oliver cites the case of two children who were seized, after drinking the liquid contained in a hollow toy—candy-anchor—with

alarming sedative symptoms, requiring active medical treatment. Enough of this liquid was secured to prove that it was flavoured with the artificial pine-apple flavour.

The artificial fruit essences appear to be extensively employed in the United States for flavouring jellies, confectionery of various kinds, and syrups for soda-water. They are also used in the manufacture of factitious wines and other alcoholic liquors. They are found to be less adapted to ices than to the other articles mentioned, and are probably used in them to a very limited extent, and not at all by confectioners of any repute.

The details given by Dr. Oliver of the mode of use of the artificial fruit essences for the above purposes are as amusing as instructive. We may note, *à-propos* of the recent appearance of sarsaparilla "fountains" in the streets of London, that the syrup from which the sarsaparilla drink is commonly prepared for these fountains in the United States is probably not unfrequently "quite innocent of the famous root." Without exception, the oils of winter-green and sassafras are added to the sarsaparilla syrup to supplement the flavourless preparation of the root. Oil of anise is said to be sometimes used in addition to the two oils mentioned.

When the essences are used in syrups, a small portion of either citric or tartaric acid is used, to give a slightly tart taste. Where also the syrup is to represent the flavour of any of the red fruits, as the strawberry, for instance, colouring matter must be added. This is generally made of cochineal. The sarsaparilla syrup, also, usually receives a little colouring, except when molasses is used, and the colouring substance is caramel.

In respect to jellies, one extensive manufacturer informed Dr. Oliver that he "formerly kept jellies from the pure fruit, but found it didn't pay, and now keeps none at all. Those now kept are made from apples, properly coloured, and flavoured with the artificial essences bought in New York. Pays the highest prices for these essences. The jellies cannot be distinguished by customers from those made of pure fruit; the latter are really no better."

In respect to the use of the essences in the fabrication of factitious wines and other alcoholic liquors, it is sufficient to quote the following extract from the business circular of an

eminent master in the trade :—"Liquors made according to the receipts given" (several of which Dr. Oliver cites) "so closely resemble the genuine that they often cannot be distinguished. They answer for all the same intents and purposes, as the flavours for preparing them are identical with those which are formed naturally during fermentation. Modern chemistry, which has by analysis discovered the existence of these ethers in wines and distilled liquors, has also found other more abundant sources for them, rendering it highly advantageous to employ the latter."

THE PRACTITIONER.

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Original Communications.

ON THE PATHOLOGY AND TREATMENT OF SHOCK AND SYNCOPE.¹

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THE assemblage of phenomena which we designate by the term "shock" is so much more frequently met with in surgical than in medical practice that it may almost seem that in writing a paper on this subject I have left the proper domain of the physician, and trespassed on that which the surgeon claims as his own. We shall hereafter see, however, that shock may occur in the course of diseases for which the physician alone is called into consultation, and it is intimately connected with fainting or syncope, a condition which is usually treated of in medical rather than in surgical text-books. So closely, indeed, are syncope and shock connected that they were considered by the celebrated surgeon, Travers,² to differ in degree rather than in kind, and we shall find it convenient to take a glance at the conditions which we find in syncope, before we proceed to examine those of shock.

¹ Read before the Abernethian Society, St. Bartholomew's Hospital.

² Treatise on Constitutional Irritation, 1826, p. 466.

I shall divide this paper into three parts. First, the injuries or impressions on the nervous system which occasion syncope and shock, and the symptoms which are observed in these conditions. Secondly, the causes of each symptom, and Thirdly, the remedies used and their mode of action. To put them shortly, these three heads are: 1. *The symptoms and causes*; 2. *The pathology*; and 3. *The treatment of shock and syncope*.

As I have already said, it will be convenient to consider the symptoms of syncope before those of shock. Having had little surgical experience myself, I shall quote very freely from the works of others; and the first case I shall give is one taken, not from a scientific work, but from the pages of a popular religious periodical. I cannot even vouch for the historical truth of the narrative, and yet I choose this case because we have records of numerous other ones which resemble it so much in one or more particulars, that we can hardly doubt the substantial accuracy of the description; and owing to the peculiar circumstances under which the events are said to have taken place, we find in this one instance all the phenomena which we would otherwise have to seek for: some in one case, some in another.

During the reign of Charles or James the Second, one of the Scottish Covenanters, named John Bruce, concealed himself from the dragoons who were in search of him at some little distance from his cottage, and his little daughter Alice was accustomed daily to visit him with a supply of provisions. One day, while on this errand, she was unhappily discovered and seized by the dragoons, who at once guessed her purpose from the food she was carrying, and declared that unless she informed them of the place of her father's concealment they would torture her with thumb-screws. She refused, and the instruments were accordingly applied. Scarcely, however, had a few turns of the screw been made, when her face became deadly pale, and she fell back insensible. The screw was at once undone, water from a neighbouring rivulet was dashed in her face, and after a deep sigh or two the paleness disappeared and consciousness returned. Again the dragoons demanded her secret, adding the threat that they would not let her off so easily this time. Again she refused, and the dragoons, irritated by her obstinacy, by a few

rapid turns of the screw nearly crushed her thumbs between the jaws of the instrument. A second time the deadly pallor overspread her face, and unconsciousness relieved her pain. This did not suit the purpose of the dragoons, and they again sought to restore her as before. But this time all their efforts were unavailing; the heart had ceased to beat, and the poor child was dead.

Here we have a typical instance, first of fainting, then of death by syncope, following the infliction of intense pain alone, without any injury whatever to the vital organs. Sometimes death may occur from an impression on the nervous system without even pain being felt, as in a case recorded by Sir Astley Cooper in his lectures on surgery.¹ "A healthy labourer belonging to the India House was attempting to lift a heavy weight, when another labourer came up and said, 'Stand on one side; let an abler man try.' At the same time he gave the former a slight blow on the region of the stomach, when the poor fellow immediately dropped down and expired. On examination of his body there was not any mark of violence discovered." This may be regarded as a typical instance of instant death from shock, but cases like it are comparatively rare. Usually the injury is succeeded by a period of depression of all the vital functions, and this may either end in death, pass into a state of excitement, or gradually disappear and give place at once to health without any intervening excitement.

The symptoms ordinarily observed in shock are well illustrated by a case which Professor Fischer has described in a clinical lecture on this subject.² From this I have made the following extracts:—"The patient, a strong, and perfectly healthy young man, was struck in the abdomen by the pole of a carriage drawn by runaway horses. No serious injury was done to any of the internal organs, at least we have not been able after a careful examination to find any trace of one. Nevertheless, the grave symptoms and the alarming look which he still presents made their appearance immediately after the accident. He lies as we see perfectly quiet, and pays no attention whatever to anything going on around him. His coun-

¹ Lectures on Surgery, from notes by Tyrrell, 1824, vol. i. p. 10.

² Volkmann's Sammlung Klinischer Vorträge, No. 10.

tenance is sunk and peculiarly elongated, his forehead is wrinkled, and his nostrils dilated. His weary, lustreless eyes are deeply sunk in their sockets, half-covered by his drooping eyelids, and surrounded by broad dark rings. The pupils are dilated, and react slowly to the light. He stares purposelessly and apathetically, straight before him. His skin and such parts of the mucous membranes as are visible are pale as marble, and his hands and lips have a bluish tinge. Large drops of sweat hang on his forehead and eyebrows. His whole body feels cold to the hand, and a diminution in temperature is readily detected by the thermometer, which indicates a degree and a half in the axilla, and a degree centigrade in the rectum, below the normal. Sensibility is much blunted over the whole body, and only when a very painful impression is made on the patient does he fretfully pull a wry face and make a languid defensive movement. He does not move a single limb spontaneously, but after being repeatedly and urgently requested, he shows that he can still execute limited and brief movements with his extremities. If the limbs are lifted and then let go, they immediately fall as if dead. The sphincters remain closed in our patient, at least passage of urine and fæces has not been noticed since he came into the hospital. When drawn off with the catheter, the urine is found to be scanty and dense, but free from any traces of sugar or albumen. The pulse is almost imperceptible, irregular, unequal, and very rapid. The arteries are small, and the tension very low. While the patient was being brought to the hospital the pulse became quite imperceptible, and the cardiac sounds very irregular and intermittent. The patient is perfectly conscious: he replies very slowly and only when repeatedly and importunately questioned, but his answers are quite to the point. You heard how he gave the details of the accident reluctantly and imperfectly, but in the main correctly. Only while he was being brought to the hospital did he refuse to answer at all. His voice is hoarse and weak, but his articulation is good. On being repeatedly questioned the patient complains of cold, faintness, formication, and deadness of the extremities. When he shuts his eyes he becomes sick and giddy. His senses are perfectly acute. His respiration appears to be irregular, and abnormally long, deep and sighing inspirations alternate with

very superficial ones, which are scarcely visible or audible. While being brought to the hospital he vomited several times, and nausea and hiccup still remain. Anyone who knew the patient, or had seen him shortly before the accident, could hardly recognise him now. His appearance, cold skin, and hoarse voice immediately recall the appearance of a cholera patient to the memory of the attentive observer; the characteristic dejections are alone wanting to make the resemblance complete."

But cases of shock do not always present these appearances. If we call the form just described the torpid one, we can readily distinguish from it another erethismic form which Travers terms "prostration with excitement."¹ The countenances of patients suffering from it are distorted, and express a nameless anxiety and excruciating agony. They toss wildly about, groan and scream, and complain of a fearful oppression and want of breath, oppressive presentiments of death, and a feeling of total annihilation. No encouragement is of any use; they lament and behave themselves like madmen. The consciousness of these sleepless and restless patients is unclouded, but seems to be completely engaged by the frightful anguish. On this account they answer no questions, but only sigh and moan. They murmur to themselves, and pay no attention to what is going on around them. Such parts of the mucous membranes as are visible are pale, but the countenance, on the contrary, is slightly flushed, and the forehead hot; the eyes are sunk, but have a peculiar lustre, and the pupils are contracted. The skin of the extremities is generally cool and insensible, but not to the same degree as in the torpid form of shock seen in the case of the patient already described. Occasionally no coldness is perceptible. Vomiting of quantities of mucus and painful retching are constant and very obstinate symptoms of this form of shock. Burning thirst is present, and liquids are greedily swallowed, but no sooner are they down than they are again rejected. Every movement is made hastily and accompanied by a remarkable trembling. Occasionally all the limbs shiver as in a rigor, and the patient has no power to control the movement. A wounded officer in this condition repeatedly requested me, says Professor Fischer, not to consider it as a sign that he was afraid.

¹ Op. cit. p. 407.

Convulsive movements, and fibrillary twitches of the muscles, and especially of those in the face, are observable. The respiration is frequent and superficial, the pulse small and cannot be counted.

Both these forms of shock may occur independently, but there is a certain connection between them. Patients recovering from the torpid form may come gradually to present the symptoms of the erethismic, and *vice versa*, when the condition becomes worse, the torpid may be developed from the erethismic form. The latter is then to be regarded sometimes as an independent condition, and sometimes as a second stage of the torpid form.

Both forms of shock sometimes terminate in death, while at other times, according to Mr. Travers,¹ instead of the continuance and fatal increase of the symptoms of prostration, they may gradually give place to a partial and defective reaction, protracting life, but scarcely improving the prospect of restoration, which remains doubtful for several days in succession; or, on the contrary, an efficient and healthy degree of reaction may be quickly established consequent upon symptoms threatening the most unfavourable issue. "Again and again," he continues, "I have left the bedsides of patients brought into the hospital pulseless, and apparently moribund without any external injury, having suffered falls or blows so serious as to have induced the symptoms of prostration to an alarming extent, and have found them on the succeeding day, to my great surprise, restored to the tone and tranquillity, comparatively speaking, of health. Reaction has in these cases been spontaneous, or nearly so, although gradual enough to occupy a period of many hours." "Now, had such persons suffered topical injuries of a severe though reparable description, it is to my mind more than probable that reaction would have failed altogether; but had it, by favour of circumstances, been established it is at least, equally probable that it would have taken the form of excitement. In other cases days have elapsed before a perfect reaction and complete relief have been obtained."

Having said so much regarding the symptoms of shock, let

¹ Op. cit. p. 409.

us shortly run over its causes. The cases already related show us that it readily follows a blow on the abdomen, sometimes even when the blow is by no means severe. Injury to the genitals is another important cause of shock. Hardly anyone finishes his school days without receiving a blow on the testicles, either at cricket or during the struggles at football, and ever afterwards he bears vividly in mind the dreadful depression, weakness, and sickness which instantly overpowered him. The same thing takes place in operations, and Mr. Erichsen¹ has observed that at the moment of division of the spermatic cord in castration the pulse sinks even though the patient has been fully anasthetised. Still more striking are the effects occasionally observed on the passage of a catheter or bougie. They are thus described by Sir Astley Cooper:² "A person has a bougie passed into his urethra for the first time; the urethra is irritated by it; he says, 'I feel faint,' becomes sick, looks pale, and, without care, he drops at your feet. His pulse has nearly ceased, and his body is covered with a cold perspiration. You place him on a sofa with his head a little lower than his body, and as soon as the blood freely enters the brain all his functions are restored. Thus, by irritating the urethra the stomach is influenced, the actions of the head and heart are suspended, and the powers of the mind vanish."

Injuries to bones have a peculiar power to induce shock. It is, perhaps, more frequently observed as a consequence of the crushing of bones in railway accidents than of any other cause whatever. It may be said that in such cases all the textures of the limb, skin, fascia, muscles, vessels, and nerves are injured as much as the bones; but two cases of Pirogoff's³ seem to show that it is to injury of the latter rather than of the former structures that the effect is to be attributed. In two amputations of the thigh which he performed, before the introduction of chloroform, death occurred on the operating table.

One case was for severe traumatic injury, the other for chronic disease of the knee-joint, which had greatly weakened the patient. In both cases the pain and loss of blood during

¹ Science and Art of Surgery, 4th edit. p. 6.

² Lectures on Surgery, from notes by Tyrrell, 1824, vol. i. p. 9.

³ Quoted by Fischer, *op. cit.* p. 10.

the operation was only a little greater than usual, yet, in both, immediately after the bone had been sawn through the face became pale, the eyes staring, the pupils dilated, a peculiar rigidity of the body occurred, and death immediately took place.

Extensive burns frequently cause shock in a marked degree, and such, says Mr. Travers,¹ is the effect of the transient bodily pain experienced in the extraction of a tooth, or the extirpation of a wart or corn, as in some persons to produce syncope, retching, or convulsions. Nor must the effect of mental emotion be left out of account, as this is sometimes sufficient of itself to cause death without any injury to the body whatever. Many years ago the janitor of King's College, Aberdeen, had rendered himself in some way obnoxious to the students, and they determined to punish him. They accordingly prepared a block and axe, which they conveyed to a lonely place, and having dressed themselves in black, some of them prepared to act as judges, and sent others of their company to bring him before them. When he saw the preparations which had been made he at first affected to treat the whole thing as a joke, but was solemnly assured by the students that they meant it in real earnest. They proceeded to try him, found him guilty, and told him to prepare for immediate death, for they were going to behead him then and there. The trembling janitor looked all round in the vain hope of seeing some indication that nothing was really meant, but stern looks everywhere met him, and one of the students proceeded to blindfold him. The poor man was made to kneel before the block, the executioner's axe was raised, but instead of the sharp edge a wet towel was brought smartly down on the back of the culprit's neck. This was all the students meant to do, and thinking that they had now frightened the janitor sufficiently, they undid the bandage which covered his eyes. To their astonishment and horror they found that he was dead.

Another case is related by Mr. Travers,² who saw a man suffering from strangulated hernia expire suddenly on the table during the steps preliminary to the operation which, from the state of the symptoms and of the bowel as ascertained by examination after death, might be said to afford the fairest prospect of relief.

¹ Travers, *op. cit.* p. 74.

² *Op. cit.* p. 23.

The cases of shock of which we have so far spoken are perhaps more likely to come under the notice of the surgeon than of the physician. The state of shock, or collapse as it is more frequently called in medical practice, occurs when the abdominal viscera are injured from within, just as when they receive a blow from without. Thus the intense irritation which corrosive poisons, such as sulphuric and other mineral acids, or large doses of arsenic, occasion in the stomach, produces, in addition to local pain, coldness and pallor of the surface, sighing respiration, and weak or imperceptible pulse. The same thing occurs when perforation takes place in the stomach or intestines, and their contents escape into the peritoneal cavity. The occurrence of shock after parturition, especially in cases of twins, is probably partly due to nervous influence and partly to the removal of pressure from the abdominal vessels by the loss of such a large portion of the abdominal contents, which must almost unavoidably occasion more or less relaxation of the vessels.

To recapitulate shortly what we have said under this head, the *symptoms* of shock are: pallor and coldness of the skin, weak pulse, oppressed and sighing respiration, dilated pupil, and sickness.

The *causes* of shock are: painful impressions—more especially extensive burns—injuries to bones, and, above all, injuries to the abdominal viscera and genitals.

We have now to consider our second head, viz.: *The pathology of shock; or, the causes of each symptom.* I ought properly to take up every one and trace it back to its cause, but I shall not attempt to do this, because it would occupy too much time, and I am not sure I could at present succeed in the attempt. I shall, therefore, be content to glance at a few of the principal symptoms only.

And first:—Why should the pulse be small and weak and the tension in the artery low, so that a slight pressure with the finger is sufficient to compress its walls and completely arrest its pulsations? The smallness of the pulse wave under such conditions at once informs us that only a little blood is sent into the arteries at each contraction of the heart. This may be owing to the heart acting so feebly and imperfectly that it only

sends out a small portion of the blood with which its cavity was filled, or it may be that it is doing its duty perfectly but has no blood to send out. It be would very hard to say which of these two causes is the true one, or whether they do not both share in the production of shock, if we had not experiments on the lower animals to give us some clue to the true solution. Several years ago Professor Goltz, now of Strasburg, found that on striking the abdomen of a frog several times the heart stopped still altogether.¹ After a short pause it again went on, but instead of becoming completely full during each diastole, and sending a large volume of blood into the arteries at each systole, it remained pale and empty; no blood at all, or hardly any, flowed into it during the diastole, and consequently it could not send any into the vessels when it did contract, and it might just as well have remained motionless. On looking for the blood that ought to have been supplying the heart, he found that it was stagnating in the vessels of the abdomen, and especially in the veins. The intestinal vessels are so capacious that when they are fully dilated they can hold all the blood in the body. Normally, however, they are kept in a state of partial contraction by the influence of the vaso-motor nerves which supply them. It used to be supposed that these nerves only went to the arteries, and that these alone were capable of contraction and relaxation, but Goltz found that the veins also were supplied by vaso-motor nerves, and that they too could contract and dilate, though to a less extent than the arteries. Whenever the power of the vaso-motor nerves was destroyed, both arteries and veins dilated and held so much blood that there was not sufficient left to keep up the circulation in the rest of the body. If the frog was held in the upright position no blood at all reached the heart, but if it was laid horizontally a little blood trickled into the heart, and the circulation was thus kept up, though very weakly.

Here, then, we have in the frog the same effects produced by a blow on the abdomen, as in the case of the young man who was struck by the carriage pole, with this difference, that in the man we could only feel the weakness of the pulse, while in the frog we can see why it is weak. Professor Fischer says that

¹ Virchow's Archiv. xxvi. p. 11, and xxix. p. 394.

the best and shortest definition of shock which has yet been given is that of Mr. Savory:—"Shock is the paralysing influence of a sudden and violent injury to nerves over the activity of the heart." The experiments of Goltz show that this definition is perfectly correct, but you must not forget that there are the two factors in shock as seen in the frog. First, the stoppage of the heart; and second, dilatation of the vessels. These are quite distinct, and I have frequently observed that blows of moderate severity will produce in some frogs stoppage of the heart without dilatation of the vessels; in others vascular dilatation without arrest of the cardiac pulsations, although severe blows generally produce both.

The pallor of the surface and the coldness of the skin are the next symptoms which engage our attention, and what we have just learned regarding the circulation will render their explanation easy. The rosy flush of health is due simply to the red colour of the blood shining through the skin as it courses through the capillaries, and whenever the circulation is stopped, either by the vessels contracting as after exposure to cold, or by the blood stagnating in the abdomen as in shock, pallor overspreads the surface. The warmth of the external parts of the body is due to the warm blood from the interior, which heats them in the same way that rooms are warmed by hot-water pipes, and whenever the circulation ceases there is nothing to prevent the surface of the body being cooled down to the temperature of the surrounding medium, and such, in fact, does take place. The lividity or blueness which is occasionally observed, is due to the blood in the capillaries becoming dark and venous as it flows sluggishly through them, or even stagnates in them altogether when the circulation is very weak. I shall at present say nothing about the respiration or sickness, but pass on to consider the insensibility which we find in syncope though not in shock, and which distinguishes the former from the latter.

The functions of the brain, on whose failure insensibility depends, require for their performance a constant supply of blood, and when this is cut off they at once cease. A year or two ago, Dr. Waller proposed to produce temporary anæsthesia for short operations by compressing both carotids, or, in fact, garotting

the patient; and I have been informed by my friend Mr. Image, of Bury St. Edmunds, that in Baron Larrey's Hôpital du gros Caillon, in Paris, it was the usual custom before the introduction of chloroform to lay a patient on his back, and then to lift him up very suddenly to the standing posture, whenever they wished to induce fainting for the purpose of relaxing muscles in cases of dislocation. The vessels of the patient were carrying on the circulation all right while he was in the horizontal position, but they had not time to adapt themselves to the altered conditions when the man was placed upright, and so the blood ran to the depending parts of the body, and the brain was left without it.

But why should a fainting fit, which, apparently, is more severe than shock, inasmuch as the brain also has ceased to act and the patient is thus rendered more deathlike, be quickly recovered from, while shock lasts for many hours? This is a question difficult to answer, inasmuch as the necessary data fail, and we are forced to fall back on hypothesis. In attempting to answer it we must remember that it is not really the heart's action which keeps up the circulation directly. It is the pressure of the blood inside the arteries forcing it on through them, and, as Goltz's experiment shows, the heart may be pulsating and yet the circulation be at a standstill. Now, the arterial tension may be lessened, (1) by the heart stopping, or (2) sending little blood into the arteries at each beat, or (3) by the arterioles dilating so as to let the blood easily out from the arteries into the veins, where it may stagnate and be useless. Another point we must remember is that it is the circulation in the brain which is the important factor in producing insensibility. In the patient who was suddenly lifted on his feet the circulation in the body was going on perfectly well; it only failed in the brain.

Now, it is very easy to bring down the blood-pressure very much by stopping the heart for a few instants, and it may take a little while before it rises to its normal condition. A second way is to dilate the arterioles, and if the arterioles be dilated at the same time that the heart is stopped, the pressure will fall with great rapidity, and, when the heart again begins to beat, it will take a much longer time to raise the pressure sufficiently to

carry on the circulation than it would otherwise do. Now, when a painful impression is made on a sensory nerve, it is not unfrequently carried up to the medulla oblongata, where it is transferred to the vagus nerve, which, as you know, has the power of stopping the heart, and by setting this nerve into action arrests the cardiac pulsations. If the arterioles should happen to be dilated, as they almost always are in a warm room, the pressure of blood in the arteries immediately sinks, the brain getting an insufficient supply ceases to act, and the patient falls down unconscious. The very fact of the head being lowered induces more blood to pass to it, and the normal condition is at once in many cases restored.

The condition of the vessels in fainting has not been ascertained, and the only observation bearing on the subject that I can find is one by John Hunter.¹ While engaged in bleeding a lady she fainted, and during the continuance of the faint he observed that the blood which flowed from the vein, instead of being dark and venous, was of a bright scarlet colour, like that of arterial blood. Now, the only condition in which we know this to take place is when the arterioles are greatly dilated, and the blood flows so quickly through them that there is no time for it to be deprived of oxygen during its passage. This is seen in the submaxillary gland during irritation of the chorda tympani nerve, and it was observed by Meyer,² the celebrated proponent of the doctrine of conservation of energy, in persons

¹ Works of John Hunter, edited by Palmer, 1837, vol. iii. p. 91.

² R. Meyer, *Die organische Bewegung in ihrem Zusammenhang mit dem Stoffwechsel*, 1845, p. 84. Meyer's explanation of the occasional red colour of venous blood is different from the one I have given above. We both agree that the slowness of the alteration it has undergone in its passage from the arteries into the veins is due to the fact that but little oxygen has been taken from it by the tissues as it flowed through the capillaries. Meyer considers that the tissues adapt themselves to the wants of the body, and take little oxygen from the blood when the external air is warm. The oxidation which usually goes on within the body is thus diminished, the production of the heat lessened, and the temperature of the animal prevented from rising too high. This hypothesis, though very plausible, is rendered improbable by the experiments of Bernard (*Revue Scientifique*, 1871-72, pp. 133 and 182), which show that the tissues of animals which have been exposed to a high temperature absorb oxygen (after death at least) much more quickly than usual. I therefore attribute the florid colour of the blood to dilatation of the arteries and capillaries, allowing it to flow so quickly through them that the tissues have not time to abstract much oxygen, however great their avidity for it may be.

whom he bled in the tropics, and who had their vessels dilated in consequence of the heat; and it was also noticed by Crawford¹ in animals bled during immersion in a warm bath. It would therefore seem that in fainting the vessels of the external parts of the body are occasionally, at least, widely dilated, and this explains the frequency with which persons faint in warm rooms and crowded churches. I am inclined then to suppose, that in fainting there is dilatation of the vessels in the *external* parts of the body, although the data on which I found my opinion are too imperfect to allow of my speaking very positively on the subject. If you examine the veins on the back of your hand in a crowded assembly, such as people often faint in, you will probably find them very full, indicating that blood is flowing rapidly into them from the arteries, and that their colour is of a lighter blue than usual, showing that the blood they contain is lighter coloured or less venous than usual. This indicates that the cutaneous arterioles are dilated, and this dilatation has doubtless a great deal to do in many instances with the reduction of the blood-pressure and the induction of syncope. As the skin is usually pallid during the fainting fit itself, we can hardly suppose that the blood is then flowing very rapidly through the cutaneous vessels. If the hypothesis I have just advanced be correct, we are thus driven to the conclusion that it is the blood-vessels of the muscles which undergo dilatation during syncope. This idea likewise receives confirmation from the observation made by Thackrah,² that it is in muscular men that venous blood most frequently presents a florid colour. Such of you as have seen a living muscle cut across, however, know that when it is at rest very little blood indeed flows from the divided ends of the vessels which permeate its substance, and you may be inclined to doubt the possibility of these vessels ever being able to dilate so much as to drain, as it were, the blood from the arteries into the veins and produce syncope. That they can dilate and drain the blood out of the arteries very quickly has been shown by Ludwig and Hafiz,³ who found that when the vessels of the intestines and skin were made to contract, the blood which

¹ Crawford, *Experiments and Observations on Animal Heat*, 1788, p. 308.

² Thackrah, *Inquiry into the Nature and Properties of the Blood*, p. 85.

³ Ludwig's *Arbeiten*, 1871, p. 107.

could no longer flow through them poured through the vessels of the muscles, and, notwithstanding the fact that these vessels were at that very time excited to contraction by irritation of their vaso-motor nerves, the blood flowed from the arteries into the veins, and the pressure in the arteries sunk nearly as quickly as when the cutaneous and intestinal vessels were putent. If such be the effect of the muscular arteries on the blood-pressure when they are trying to contract, what must it be when they are ready to dilate? Dilatation of the vessels alone may sometimes be sufficient of itself to lower the blood-pressure to such an extent that fainting occurs; but at other times this is combined with the depressing effect of sudden stoppage of the heart. In shock there is great dilatation of the vessels in the *interior* of the body, especially in the veins of the intestine. If this state should be associated with sudden stoppage of the heart, instant death will occur, as in the case of the labourer in the India Docks. In short, then, I consider syncope to depend chiefly on dilatation of the arterioles, especially of those near the surface, though in cases like that of Alice Bruce it may be due entirely to stoppage of the heart; while the chief factor in shock is dilatation of the abdominal veins. The longer duration of shock than of syncope is probably due to the veins recovering their contractility more slowly than the arterioles.

Having thus formed some sort of idea regarding the pathology of syncope and shock, we come to our third head, viz. the question of treatment. In syncope, our first idea is to restore the circulation to the brain, and this we do by laying the head level with the body, or perhaps, still better, as recommended by Sir Astley Cooper, on a level somewhat lower than that of the body.

The next thing is to raise the blood-pressure. Now, this is most easily done by causing the arterioles to contract. We therefore hurry a person who has fainted from the warm room to the cold air, and dash cold water on the face, in order to cause contraction of the vessels on the surface of the body. We also give draughts of cold water to cause contraction of those of the stomach. Besides this we apply ammonia or aromatic vinegar, which is strong acetic acid, to the nose. Why do we do this? Many of you know that when ammonia is applied

to the nose of a rabbit it causes the heart to stop instantaneously, and one would therefore think that to hold it before a fainting person's nose was to do the very worst possible thing. But we all know that this is not the case. Some time ago a member of this society asked me this question, and I could at that time give him no satisfactory answer. I have since made some experiments on the subject, and I find (what has indeed been already noticed by Kratschmer¹) that when ammonia or strong acetic acid is held before the rabbit's nose, it causes contraction of the arterioles. Consequently it prevents the blood-pressure from falling quickly, even should the heart become feeble or stop, and is thus useful in preventing syncope. When the blood-pressure has already become lowered by the occurrence of syncope, contraction of the arterioles causes it to rise, and it is by causing this that acetic acid or ammonia are useful as restoratives.

In shock we have two conditions to remove. The first of these is the feebleness of the heart itself, which is due to the action of the vagus. To counteract this we apply stimulants. Now, one of the most powerful stimulants to the heart is heat. It is true that it dilates the vessels, but in shock we have nothing to fear from dilatation of the vessels near the surface of the body, where circulation is hardly going on at all, nor is it likely that it will increase the dilatation of those in the interior. We therefore pursue a plan of treatment directly the opposite of that which we employ in fainting, and apply warmth instead of cold to the surface, especially to the cardiac region, over which a hot poultice or india-rubber bottle filled with hot water should be placed. At the same time, and for a similar purpose, we give brandy and ether internally. The second and most important indication for treatment is to cause contraction of the great vessels, especially the veins in the abdominal and thoracic cavities, so that the blood, instead of stagnating uselessly in them, may be sent onwards to the heart, and thence to the rest of the body. I have already described the effect of acetic acid and ammonia held before the nose, but this is only one example of the general rule that all powerful impressions on sensory nerves cause contraction of the blood-vessels. Painful impressions made upon the skin, for example, have this effect, and Goltz has

¹ Wiener Sitz. Bericht, 1870, Abt. II. lxii. p. 24.

shown that pinching the toes of a frog, or irritating them by acetic acid, prevents the vessels from dilating when the abdomen is struck, or causes them to contract and propel the blood to the heart if dilatation has already taken place. If I may judge from my own experience, persons not unfrequently take unconscious advantage of this effect of pain, and medical students occasionally prevent themselves from fainting, when witnessing an operation, by biting their lips or pinching their fingers. Its beneficial action in shock is very great, and my friend Dr. Fayrer informs me that he has succeeded in recovering a patient from a state of collapse by thrashing his feet and the calves of his legs with switches after other means had failed. Mustard plasters are often applied for a similar purpose. Sometimes the performance of an operation during shock is attended by a marked improvement in the patient's condition, and it seems to me not improbable that this is due to the stimulus thus given to the vasomotor nerves. At other times, however, the additional injury seems to produce an injurious effect either on the heart or vessels, and the patient succumbs. It is possible that the different effects of operations performed during shock may depend to some extent on the greater or less amount of irritation which is occasioned to the nerves of bones as compared with those of the soft parts; for, as we have already mentioned, injuries to bones tend to cause syncope, while irritation of other nerves, unless it be excessive, tends to prevent it by raising the blood-pressure. This, however, is a question which pertains more especially to surgeons, and with them I will leave it. I must not conclude without mentioning another valuable remedy in cases of shock, viz., digitalis. It has, I think, been conclusively proved by Dr. Adolf B. Meyer and myself,¹ that this drug possesses the power of contracting the arterioles, and I have shown² that it greatly strengthens the pulsations of the heart. We would therefore expect it to prove useful in shock, and experience does not disappoint our anticipations. This is well shown by a case of shock following parturition, in which it was employed by Dr. Wilks³ some years ago. My attention was drawn to this by my friend

¹ *Journal of Anatomy and Physiology*, Nov. 1872, p. 134.

² *On Digitalis*. London, 1868, p. 28.

³ *Medical Times and Gazette*, Jan. 16, 1864.

Dr. Milner Fothergill, and I quote the following from his admirable essay on digitalis.¹ "The patient was apparently *in articulo mortis*; her limbs were cold, her body in a state of deathly clammy sweat; the face was livid, no pulse could be felt at the wrist, and a mere fluttering was heard when the ear was placed over the region of the heart. Brandy and ether had been tried without any good effect, and as dissolution was imminent, it was determined to try digitalis. Half-drachm doses were given every hour: after four doses a reaction took place, and after seven doses complete recovery occurred." Such a case as this needs no comment, and a consideration of the encouraging results here obtained can hardly fail to gain for digitalis a much more extensive application in cases of shock than it has hitherto received.

¹ Digitalis: its Mode of Action and its Use. London, 1871, p. 63.

X. Lunt.

ON THE DOSES OF REMEDIES RECOMMENDED IN THE PHARMACOPŒIA.

BY THE EDITOR.

A VERY important subject has been suggested for the consideration of the profession by the result of a coroner's inquiry into a death which occurred lately. A gentleman of Ramsgate was affected with *delirium tremens*, and his medical attendant prescribed a draught containing half an ounce of tincture of digitalis, to be repeated, if necessary, in four hours' time. The chemist to whom the prescription was taken refused to dispense the dose; the patient died, and an inquest was held on the body. The medical man expressed much indignation at the chemist's refusal to dispense the medicine ordered, and intimated his opinion that the deceased might have recovered had it been given to him. The chemist justified his conduct on the ground that the dose was eight times as large as the highest dose of tincture sanctioned by the British Pharmacopœia. The surgeon stated, however, that half-ounce doses, and even much larger ones, had been given successfully by many practitioners, and that this fact was well known in the profession. The jury returned a verdict of "Death from natural causes, accelerated by drinking," and censured the chemist for not dispensing the prescription ordered.

A great deal of comment and rather angry discussion has arisen about this case; and the medical journals have been somewhat severe in their criticisms of the chemist's conduct. Certainly it does appear that the proper course would have been for the chemist to communicate his doubts about the prescrip-

tion to the prescriber, and ask whether he had really intended to give so large a quantity of digitalis. In so far as he neglected to take this step, he was decidedly wrong. But we think that the profession would be wrong to place the whole blame upon the chemist. The chief fault rests, as it appears to us, with the compilers of the posological list of the Pharmacopœia, who, by indicating 30 minims as the extreme dose of tincture of digitalis, have been guilty of an unpardonable blunder which might well mislead the dispensing druggist. The medical man who prescribed the medicine justly remarked that the propriety of half-ounce doses was supported by a very large mass of respectable medical evidence; and that it was a great hardship that the patient should lose an important chance of recovery because a chemist thought that such doses would be dangerous. The matter is all the more important because this is by no means a solitary instance of extraordinary ignorance or negligence on the part of the officials who have compiled the list of doses of different drugs published in the British Pharmacopœia. In fact, the evil is so serious that we propose to call attention to a number of instances in which it is easily apparent to a careful observer.

Let us take first the case of hyoscyamus, of which we find that the Pharmacopœia recommends the tincture to be given in doses of 15 to 60 minims. The smaller of these quantities is probably completely without effect upon the human organism; and as to 60 minims being the maximum dose, anyone in the least familiar with asylum practice must be aware that as much as half an ounce is frequently required by, and given with good effect to, maniacal patients. In the case of belladonna, the official scale of doses is deficient in both directions: 5 minims is far too large to be assigned as a minimum, and 20 minims is a long way from being the largest dose that may be given with benefit in certain conditions. The official doses of the extract only range from $\frac{1}{4}$ to $\frac{1}{2}$ grain, a still more obvious absurdity to anyone who is familiar with the varied circumstances under which belladonna may be usefully employed.

Take next the instance of conium: what sort of value can be attached to the statement that the succus conii may be given in doses of from 30 to 60 minims? It has certainly been known,

for many years past, that 4 or 5 drachms may often be given without the production of any marked physiological phenomena ; and it is only to-day that a physician of large experience in the treatment of nervous disease reported to us that he had been administering *ounce* doses of a succus made by one of our very best manufacturing druggists, without producing any recognisable effect whatever.

Again, look at the official dose of potassium iodide—2 to 10 grains : what kind of guidance does such a direction afford to the student or the practitioner ? Surely it had been discovered long enough before the publication of the last Pharmacopœia, that doses three and four times ¹⁰ as large as the highest of those just mentioned might be given, not only with impunity, but with the most remarkable benefit, in many cases of tertiary syphilis and of lead-poisoning. Now these are cases in which a timid and feeble style of administration is very far from being a trifling evil : for the affections in question are often most dangerous, and urgently require to be treated with rapidity and efficiency.

Take next the instance of quinine (we are purposely selecting examples of remedies that are in constant and familiar use) : the official dose of the sulphate ranges from 1 to 5 grains. It is unnecessary to remind any well-informed practitioner that in severe intermittent and septicæmic fevers, and also for the specific purpose of reducing pyrexial temperatures, it is idle to talk of a maximum dose of 5 grains : 30 would have been nearer the mark, but even larger quantities than this, in repeated doses, have been recommended by high authorities.

If the examples above given seem to indicate a lamentable timidity on the part of the compilers of the Pharmacopœia, there are others which show quite a different tendency, in fact an unaccountable rashness in recommending high doses of very powerful poisons. Who could have suggested such a range of doses of tincture of *nux vomica* as 10 to 30 minims, and of the extract of the same as $\frac{1}{2}$ grain to 1 grain ? The person, whoever he was, must have been entirely unfamiliar with the practical use of the drug, or he would have known that much less than 30 minims of the tincture would half tetanise many patients, and that it is very rarely advisable to exceed 10 minims ; this is

a matter on which we speak with great confidence from a large experience of the use of *nux vomica*. The official dose of strychnia is not quite so extravagantly wrong; yet $\frac{1}{30}$ grain is at least twice as high as the minimum dose should be fixed, and $\frac{1}{15}$ grain is a quantity far too large to be given in repeated doses (as strychnia usually is given) with advantage or even safety. So again with morphia: the acetate and hydrochlorate are recommended to be given in doses of from $\frac{1}{8}$ to $\frac{1}{2}$ grain; the former of which is excessive as a minimum dose even for adults, much more for children.

We could go on, for a considerable time, multiplying these examples of an insufficient and often mischievously misleading official posology: and we cannot but remark that the occurrence of such errors in works of official authority are exceedingly to be regretted, for more than one reason. That the result may be highly inconvenient in a practical point of view is shown by such an occurrence as that which led to the above-mentioned inquest at Ramsgate; but there is another point of view in which the official neglect of accurate attention to dosage is of even more unfortunate significance. We fear that it does but indicate an ignorance and a carelessness which are very widespread in the profession, and of which we are constantly encountering proofs in prescriptions which come under our notice. It is not too much to say that the majority of practitioners—consulting and general—seem to be almost wholly unaware of the important differences of effect which may be produced by extending the range of variations of doses, and especially by so breaking up one large dose into many small ones as to ensure its passage into the circulation without exciting inconvenient and unnecessary disturbance. We believe that no greater service could be performed by the colleges, or the great medical societies, than the formation of a committee of competent men for the special investigation of this question of dosage: for it is a subject which is as yet only in its infancy, and the best knowledge which exists about it is undoubtedly confined to a very small section of the medical profession.

A CASE OF RHEUMATIC FEVER TREATED WITH A COLD BATH; DEATH OCCURRING IMMEDIATELY ON LEAVING THE BATH.

BY SYDNEY RINGER, M.D.

THESE short notes are published as this case will help to answer the following questions:—Can cold baths be administered in rheumatic fever without danger? and is it advisable before employing this treatment to wait for the onset of hyperpyrexia? or should we commence it when high fever, absence of joint-pain, suppression of perspiration, and delirium show that there is danger that hyperpyrexia may occur? As hitherto all cases of rheumatic hyperpyrexia have proved fatal unless treated by cold baths, it is obvious that this case in no way contra-indicates that treatment on the occurrence of this dangerous condition.

A young woman, aged 24, was admitted into University College Hospital with rheumatic fever. Her father died suddenly from some unknown cause. Four years before the patient suffered from a severe attack of rheumatic fever. Her present illness begun about a week before her admission into hospital. On her admission she suffered from a sharp attack of rheumatic fever; her temperature rising daily to 103°. There was not, however, much joint affection, and at first she perspired freely, but latterly her skin grew dry. She rapidly got worse: thus during the nine days she was in hospital her temperature rose daily till it reached 105°, and her respirations rose from 32 to 60; her pulse remained about 120 per minute, and throughout was strong. Latterly she suffered from dyspnoea, and subsequently was propped up in bed with pillows. She wandered

a little at night, and on the day the bath was employed her intellect was a little obscured, and she passed her urine under her. At 7.42 P.M. of the ninth day of her admission she was placed in a general bath of 92°, her temperature in the axilla being 105° Fahr. In seven minutes, and before the temperature of the bath was reduced, her rectal temperature was 105°·8; the temperature of the bath was then reduced. In eighteen minutes after the commencement of the bath her rectal temperature was 105°·4. After forty-four minutes her temperature had fallen to 103°·4, the temperature of the bath being 69°. Whilst in the bath she took 4 ozs. of brandy. She was removed because her breathing grew rather shallow. After being put to bed she merely gasped a few times for five minutes and died, notwithstanding the employment of artificial respiration, energetic friction to the surface of the body, and anal injections of brandy. At the post-mortem examination we found a few patches of recent lymph on both lungs, but not an unnatural quantity of serosity in the pleuræ. The heart was universally adherent to the pericardium, the adhesions being tough; the blood in the heart and great vessels was very dark-coloured fluid and free from clots. The left ventricle and auricle were dilated—especially the auricle. On the tricuspid, mitral, and aortic valves, numerous minute vegetations were seen at the usual places. The mitral, aortic, and pulmonary valves were a good deal thickened. The mitral valves admitted three fingers nearly to the knuckles; the two segments were united for a short distance; they permitted some regurgitation when tested at the tap. The heart's substance looked healthy, and was of fair consistence. On the surface, at places, there was a thin line of paler and rather opaque tissue. The walls of the left ventricle at the base were $\frac{1}{2}$ inch thick, at the middle $\frac{5}{8}$ inch, and at the apex rather less than $\frac{1}{4}$ inch. The brain, liver, spleen, kidneys, stomach, and intestines were healthy. During life her urine contained a trace of albumen.

ON A CASE OF PERICARDIAL EFFUSION, IN WHICH PARACENTESIS WAS PERFORMED.

BY THOMAS BARLOW. B.Sc., M.R.C.S.

Physicians' Assistant at University College Hospital.

THE following case presents several points of interest. Twice the pericardium was tapped by the aspirator, and twice the abdomen, with marked relief, and without any bad effects from the operation. The child was feverish throughout, and neither the fever nor the considerable ascites could be satisfactorily explained during life, but after death these were found to be due to tubercular peritonitis. There was a considerable amount of recent and old tubercle in the abdominal cavity; whilst only a small patch, with scattered recent grey granulations, and situated in the lower lobe of the right lung, was discovered in the chest. The visceral and parietal pericardium was enormously thickened, and yet the two surfaces were not at all united, the heart hanging free in its thickened envelope.

H. V., aged 6 years, was admitted into University College Hospital, February 7th, 1873, under Sir William Jenner. He was afterwards under the care of Dr. Ringer. The boy's mother gave the following history:—At the end of November 1872 he complained one night of pain in the left knee. His mother states that there was neither redness nor swelling, that he was not feverish, did not sweat, and that his urine was not high-coloured. The pain in the knee continued for a week, but was not so bad as to prevent his moving about. In three weeks the child seemed all right again, but fourteen days ago, *i.e.* six weeks after the onset of the knee trouble, the mother noticed

that he was losing flesh, that his breath was short, his abdomen increasing in size, and that there was slight puffiness below the eyes. During the last week the child had wheezed and coughed, and his urine had contained a red deposit. Previous history:—Never had scarlet fever nor rheumatism, so far as mother knows.

STATE ON ADMISSION. — The boy walked into the ward without showing any distress. When he sits up in bed he raises his shoulders and bends his head forwards. When he lies down he lies on the left side: says he has no pain. He has an occasional short catching cough. No expectoration. A little wheezing is audible close to the bed, and there is slight dilatation of the *alæ nasi*. Pulse, 152; respiration, 40; temperature, 101°6; skin dry; tongue very red. He is very pale; skin transparent. Is slightly puffy beneath the eyes.

Chest shows rickety enlargement of the ends of the ribs at junction of the cartilages. Elevation in excess. There is recession of intercostal spaces in both inframammary and lower axillary regions. A little cedema of chest walls. Rhonchal fremitus to be felt over both fronts. There is some bulging in cardiac region. Apex beat cannot be seen nor felt. Absolute cardiac dullness begins in second space; its right margin extends to $\frac{3}{4}$ in. to the right of the sternum. Double grating friction is heard over the sternum, and $\frac{1}{2}$ in. to the right of the sternum, from the junction of the xiphoid to the junction of the third costal cartilages; loudest at the junction of the xiphoid. Neither heart sounds nor friction can be heard at or below the nipple. Right front resonant. Both backs a little deficient at bases. Vocal fremitus to be felt down to extreme bases. Pectoriloquy over both backs. Sibilant and subcrepitant râles over whole front and back.

Abdomen.—There is a small umbilical protrusion, which his mother affirms has come on during the last week. Flanks bulged, but tympanitic. There is cedema of the abdominal walls. No cedema of ankles.

Urine scanty; acid; specific gravity 1034; large deposit of lithates; no albumen, no sugar.

One week after admission the boy was seen by Sir William Jenner, who gave the following note:—"There is some fluid in the peritoneal cavity. Liver dullness begins at fifth space.

Lower border sharp and well defined—can be felt half-way between ensiform cartilage and umbilicus, and 2 in. below the margin of the thorax in the nipple line. The cardiac dulness now extends to within one finger's breadth of the right nipple. There is relative dulness from the junction of the first costal cartilage with the sternum, nearly as far as the angle of the left scapula. Friction heard as high as the second space. No heart sound to be heard in the position of the apex, but very obscure friction. Rhonchus as before; the boy expectorates a little aerated mucus." During the remainder of February and March the boy's physical signs changed little. The friction became almost inaudible, except at the ensiform cartilage, where it was very distant. Bronchitis became more pronounced, gurgling râles being heard down both backs. The ascites was unaltered. His dyspnoea became more decided, so that he was always obliged to be propped up. The jugulars became distended, although they did not fill from below.

His temperature during February and March was about 99° in the morning and 100° to 101° in the evening. He had emaciated considerably. There was no albumen in the urine, except on one day a trace: the quantity of urine in twenty-four hours was about 10 oz. His cardiac dulness (absolute) was now bounded by a line taken from a point $1\frac{1}{2}$ in. to the left of the right nipple, up to the junction of the second right costal cartilage with the sternum, thence to a point $\frac{3}{4}$ in. to the left of the left nipple.

When patient leans forward the apex can be felt very obscurely in the nipple line in the fifth space; cannot be felt when he leans back. There is still a little obscure friction at the ensiform cartilage. The first sound just audible in position of apex, but exceedingly distant.

Dr. Ringer, who had now charge of the case, being of opinion that there was no reason to expect that anything would cause the absorption of the fluid, considered that paracentesis of the pericardium was the right thing to be done. This was performed by Mr. Christopher Heath, on the 18th of April, Dr. Anstie being present, as well as Dr. Ringer.

The skin was punctured by the smallest aspirator trocar, 1 in. below and $\frac{3}{4}$ in. inside the left nipple. The trocar was

pushed up and out. It entered the pericardium above the fourth rib: $3\frac{1}{2}$ oz. of fluid were withdrawn, and then it ceased to flow; the trocar was pulled out, and the point where it had been introduced was covered with collodion. The fluid was slightly turbid, brownish in colour; sp. gr. 1025; showed nothing under the microscope except a few blood-corpuscles and large epithelium scales, and coagulated *en masse* on boiling. There were no signs of collapse during the operation; the pulse was unaffected. Although the quantity of fluid removed was so small, the symptoms were decidedly relieved. He lay with his head on the pillow, which he had been unable to do for weeks, and his respirations were reduced from 56 to 48 in the minute. With respect to physical signs: immediately after the operation there seemed a little improvement in the note under the left clavicle, but when examined later in the evening by Dr. Ringer, not a finger-breadth's difference in dulness could be detected in any direction. The apex beat was certainly more distinct, being felt in the nipple line 1 in. below the nipple; also $\frac{3}{4}$ in. outside and below the nipple.

Next day a little superficial rhonchus was heard, along with gurgling and subcrepitant rhonchus over the left front. It simulated pleuritic friction, but altered slightly on coughing. From the time of his entering to that of leaving the hospital there was no sign of effusion into the pleuræ.

The boy's improvement did not last more than two or three days, and on the 26th April it was decided to do paracentesis pericardii again. It seemed probable that the small quantity of fluid removed on the first operation was due to the trocar needle having been introduced too high and directed upwards. Mr. Heath therefore on this occasion punctured directly through the skin into the pericardium at the upper border of the fifth rib instead of the fourth. The canula was pushed in two inches deep: 6 ozs. of reddish brown fluid were withdrawn; sp. gr. 1024, neutral; coagulated *en masse* on boiling.

After the operation the resonance was decidedly improved on the right of the sternum. The position of the heart's apex was unchanged, but it was felt more distinctly both in the nipple line and to the left of the nipple.

The boy's symptoms were again relieved. He was able to lie

down, and slept that evening. There was less jugular pulsation : the pulse and respirations did not change ; they averaged about 150 to 50 respectively.

His temperature in the evening now ranged from 101° to 103° , in the morning from 100° to 101° .

In two days after the operation the cardiac dulness was as great as before ; and now the abdomen, which had hitherto been excessively distended but had always been resonant in the flanks, became quite suddenly dull throughout, and so prominent in the umbilical region that it seemed likely to burst if not tapped. Now also there was considerable cedema of the legs and thighs.

Paracentesis abdominis was performed by Mr. Heath on the 2nd May, and 35 ozs. of clear slightly green fluid withdrawn, which coagulated spontaneously after standing a few minutes. Five days after this a decided diminution in cardiac dulness was noticed. The absolute dulness no longer extended beyond the right margin of the sternum, but was not otherwise affected.

The apex beat, however, was felt for a distance of an inch inwards from the nipple line in the fifth space, and well marked ; a rubbing sound was to be heard both below the nipple and at the ensiform cartilage.

The lung signs remained as before. The ascites soon reaccumulated, and the tricuspid regurgitation became more marked. Not only in the jugulars, but also in the veins on the back of the hand, pulsation of respiratory rhythm was to be seen ; and on one occasion Sir William Jenner believed there was a finer pulsation to be seen in the veins which corresponded with the heart's beats.

The boy was passing at this time not more than 12 ozs. of urine in twenty-four hours. He now began to get paroxysmal attacks of dyspncea, during which he became quite purple ; and the ascites having reaccumulated, paracentesis abdominis was repeated.

On May 23rd 36 ozs. were withdrawn, of the same character as before. This relieved his dyspncea considerably. His temperature at this time was lower than it had been before, namely, 101° in the evening, and $99^{\circ}6$ in the morning. On one occasion the morning and evening temperature was $97^{\circ}8$.

On the 2nd of June the boy's mother, being afraid of his

dying in the hospital, took him out. I saw him at his home two or three times a week until he died. The general cedema increased immensely. The right thigh, in spite of punctures, sloughed in two places (which were not the seat of punctures). One of these sloughs separated down quite close to the bone.

The boy died on the 3rd of July, *i.e.* seven months after the commencement of his illness.

POST-MORTEM EXAMINATION.—The pericardium contained half a pint of straw-coloured fluid. The parietal and visceral layers were covered with lymph $\frac{1}{2}$ inch thick. This lymph posteriorly presented quite a honeycombed appearance. Heart substance normal to naked eye inspection. Orifices normal except the tricuspid, which was dilated. There was about an ounce of fluid in the right pleura. In the upper part of the lower lobe of the right lung a mass of racemose tubercle, becoming cheesy, but not yet softened; in the lower part, grey granulations diffused throughout. No cavities. The upper lobe healthy. No granulations on the pleura. Bronchi full of mucus; walls injected. Left lung was pushed back, but not collapsed, and was to naked eye appearances healthy. No fluid in the left pleura. Mediastinal glands enlarged and matted together, firm and spotted on section, like those commonly described as tuberculous glands. Abdominal cavity showed abundant grey granulations in the mesentery and on the parietal peritoneum. Liver partly adherent, markedly nutmeg. A large blood cyst composed of semi-decolorised clot was seen between the liver and the stomach. No explanation of this could be offered, except that it occurred on one or other of the occasions when paracentesis of the abdomen was performed. Spleen healthy. Kidneys healthy with the exception of two little tuberculous (?) spots just under the capsule. No ulceration of intestines, and nothing abnormal about mesenteric glands, except that a few were soft and swollen. Permission not granted to open the head.

SOME OBSERVATIONS ON THE USE OF PHOSPHORUS IN NEURALGIA.

ILLUSTRATED WITH TWENTY-THREE CASES.

(*Second Paper.*)

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IN a former paper on this subject it was said that 1,000 drops of absolute alcohol would dissolve ten grains of phosphorus. This is not quite correct, the fact being that it takes 242 grains of alcohol to dissolve one grain of phosphorus. The tincture with which most of the former series of cases were treated was made in these proportions. A watery mixture made with this tincture, although far less disgusting than an emulsion of oil of phosphorus, is still disagreeable enough to cause many patients to refuse it; it produces a very objectionable though diminished amount of eructation; and, lastly, it is very unstable. Although these objections are perhaps to be surmounted by the use of pills or capsules, the greatly superior result, which I believe I have seen to follow the use of a solution of the pure drug in a condition which admits of more easy absorption than those forms, rendered it necessary to discover some other medium of administration; and I have found that the following prescription answers all requirements:—

FORMULA No. 3.

Phosphorus	1 grain.
Absolute alcohol	5 drachms.
Glycerine	1½ ounces.
Spirit of wine	2 drachms.
Spirit of peppermint	2 scruples.

Let the phosphorus be dissolved in the alcohol with a little heat; at the same time warm the spirit and glycerine together. Mix the two solutions while hot, and add the spirit of peppermint on cooling. One drachm of this mixture contains one-twelfth of a grain of pure phosphorus. These ingredients form a mixture perfectly bright and clear, possessing almost no phosphoric odour or taste, and of a high degree of stability even under exposure to light. The amount of spirit gives it a burning taste which may be sometimes objected to; but if the patient be warned of this, probably no further remark will be made about it. So far from causing offensive eructations, it seems to have a tendency to arrest existing flatulence.

A remark of Dr. Radcliffe's referred to above, to the effect that the disappearance of the characteristic odour from a solution of phosphorus is presumptive evidence of the oxidation (and therefore, I suppose, of the therapeutic inertia) of the drug, deterred me from publishing this formula until it had been well tested. Reference to the table of cases below will show that thirteen patients have been successfully treated with it; and in proof of its stability it is only necessary to cite Case 27, which recovered under the remains of a mixture with which Case 25 had been treated two months before.

ZINC PHOSPHIDE.—I have given zinc phosphide in six of the tabulated cases, viz., Nos. 19, 23, 24, 35, 36, and 37. This preparation is said to contain one-fourth of its weight of pure phosphorus, of which only a half is available for therapeutic purposes. In order, therefore, to administer one active part of phosphorus, it is necessary to give eight parts of the compound. Practical experience seems to confirm this statement, and I have found two-thirds of a grain of zinc phosphide (the therapeutic equivalent of one-twelfth of a grain of phosphorus) the most efficient dose. The material is conveniently made into small pills, which do not seem liable to deterioration from keeping; and it has the great advantage of admitting of combination with any other remedy which may be deemed advisable in the case. It seems to take a longer time to effect cure, or even to alleviate the pain, than either the oil or tincture of phosphorus, but there is this difference between its action and theirs—that when alle-

viation of pain is observed, the cure generally proceeds with rapidity, while during treatment with either of the solutions I have frequently observed very speedy alleviation followed by a comparatively protracted cure. Of the six cases alluded to, it will be seen that two recovered perfectly ; two, notwithstanding very adverse circumstances, were remarkably relieved during the short time they were under treatment ; while of the remaining two, one was unable to retain the pills, which in this case caused vomiting,¹ while the other found no relief in seventy-two hours, though amenable to the remedy in another form. It will thus be seen that the four successfully treated cases do not present any instance of such rapid cure as is included in any series of four consecutive cases treated with a solution of phosphorus.

Occasionally this preparation causes nausea or even vomiting. This happened to the patient in Case 35, who took eight pills of two-thirds of a grain each ; vomiting two or three times after the sixth dose, and again after the seventh and eighth. But she was already a little relieved by the remedy, and very quickly recovered under F. No. 3. In Case 24, on the other hand, although the pills were taken with exact regularity during seventy-two hours, no effect of any kind could be traced to them. No vomiting occurred, nor any relief. He recovered after the third dose of F. No. 1, which I then prescribed for him, *i.e.* in twelve hours ; and perhaps the phosphide had paved the way for such a very rapid cure. I was quite unable to assign any reason for the apparent inertia of the compound on this occasion—the only one on which it has apparently quite failed. In other cases in which nausea has followed its use, that has always been easily avoided by following the usual direction in such cases—that the dose should be taken into a full stomach ; and I only remember two other cases in which actual vomiting occurred. Although I am not able here to present the result, I have lately used this preparation both in neuralgic and other cases much more frequently than any other. The cases in which phosphorus is appropriate are for the most part such as do not require confinement to the house ; and the convenience of a remedy which can be carried in the pocket and taken without preparation in any convenient interval of business, is therefore

¹ *Vide* remarks on Cases, *infra*.

very great. It alone, moreover, as remarked above, has the valuable advantage of lending itself to admixture with various adjuvant drugs, as quinine, aloes, or strychnia. On the other hand, F. No. 3 is better adapted to young persons and some adults who cannot or will not swallow pills, and is taken by the former with pleasure.

In the two cases in which I first gave phosphorus in this form, I inadvertently prescribed a quantity equivalent to only one seventy-second of a grain. One was that of a lady suffering from chronic gastritis, the other that of a young man debilitated by excessive mental occupation; neither of them had ever had neuralgia, although they both appeared to me to be favourable subjects for the disease; indeed, the second is the brother of Case 40. After these two patients had taken respectively nine and seven doses, they complained of severe frontal headache accompanied by frequent stabs of pain, apparently darting from before backwards to the occipital region, but intracranial, and not attended by any disturbance of sensation in the scalp. These symptoms appeared to me to be those of neuralgia, or neuralgic at least; and the second case so named them from his sister's description of her sensations. Under a dose equivalent to one-twelfth of a grain of phosphorus, both patients very quickly recovered and have had no return of pain. These are the only two cases in which, under favourable circumstances of experiment, I have observed any apparent confirmation of the homœopathic hypothesis, and I will not attempt to explain these phenomena from that point of view. But since after its first prescription by Dr. Radcliffe the use of phosphorus in neuralgia was practically reintroduced to notice under auspices of homœopathy,¹ it may not be out of place to remark here that it holds no more distinguished a place in the homœopathic pharmacopœia than the other thirty-and-odd drugs which are there recommended in the treatment of this disease. It would be tedious to enumerate those medicines here, and some diffidence too is natural in meddling with so formidable an arsenal.

I conclude, then, with regard to phosphide of zinc, that it is inferior in power (in neuralgia) to the oily solution or the tincture; while, in point of convenience—for portability, for

¹ Bradley, *Brit. Med. Journal*, Oct. 26, 1872.

stability, and for combination with other drugs—it is superior to them. In point of cost it stands far above comparison with pills and capsules, which require a special process for their manufacture, while it is not subject to any suspicion of degeneration with age.

SODIUM HYPOPHOSPHITE.—There are three cases in this series, Nos. 28, 29, and 30, which were treated in the first place with hypophosphite of soda. There was nothing remarkable to be noted in the symptoms of any of them, although they are detailed below as tending to illustrate the value of this preparation. The dose given varied from five to thirty grains, repeated every three hours; it was administered in water, with which it makes a not unpleasant saline solution without any phosphoric odour or taste. In none of these cases was any relief afforded which could fairly be attributed to this remedy, although in one of them there was a trifling alleviation for two days; but as after this the pains returned with their former severity, the intermission was probably due to other influences. This compound does not appear either in the British Pharmacopœia¹ or in Dr. Garrod's "Materia Medica" (third edition); but taking the initial dose from Dr. Radcliffe (*loc. cit.*), I increased it in these cases to as much as half a drachm given every three hours. The result of these experiments was not good enough to invite or even to justify a repetition of them; they were all made on fair uncomplicated specimens of neuralgia, and, while the hypophosphite was entirely without effect, two were shortly cured and one very much relieved by a solution of pure phosphorus. Still it is possible that a much larger dose might not be without effect. The preparation, however, is a pleasant and convenient one; and probably where the object to be attained is the restoration to health of a patient suffering from extreme debility, whether accompanied or not by special (nerve) symptoms, this will be found the most convenient form for combination with other tonics and for long-continued administration. I have found it serviceable in such cases in the smaller doses; and I believe it has a special power of creating appetite. It seems to be a

¹ Neither does this work recognise the internal administration of pure phosphorus, as it was stated to do in the former article.

remedy which, to maintain its influence, requires to be given in progressively increasing doses.

As has been remarked above, Dr. Radcliffe recorded (in 1863) the successful treatment of four cases of nerve pain with phosphorus in this form; while Dr. Anstie, on the other hand, has not found it a satisfactory remedy. These somewhat conflicting observations suggest the possible instability or varying quality of different specimens of hypophosphite of soda. Of the three cases here noted, No. 29 was treated with some of this drug known to be six years old, and which presented all the more obvious characteristics of the recent preparation which was given to Nos. 28 and 30. But the evidence of stability afforded by these experiments is of course purely negative.

(The table of cases, continued from July number, will be found on the next page.)

NOTES OF SPECIAL CASES:—This series of twenty-three cases fully bears out the remarkable success which attended this treatment of the eighteen cases reported in a former number of this journal. Among them there are only three (Nos. 28, 36, and 37) in which perfect recovery did not result; and in one of them (No. 36) at least it was scarcely reasonable to expect any very marked improvement under the circumstances of it. The patient was a poor woman, thirty-three years of age, in an indigent condition of life, and imperfectly fed and clothed; she had borne children rapidly, and was still suckling the youngest at fourteen months. She had suffered from severe leucorrhœa, from dyspepsia, and from neuralgia of medium severity, for six months. She was now therefore in a condition of debility from which long treatment under favourable circumstances could alone retrieve her, and it is not surprising that a few doses of zinc phosphide should fail to cure a secondary symptom of more serious disorders. Yet she was decidedly relieved by the remedy when in five days' time she found means to go into the country.

Case 37.—This young woman had suffered from migraine for the last five years—since the beginning of menstruation. The attacks were in the habit of returning every two or three weeks, or upon occasion of the least excitement or over-exertion. She took two-thirds of a grain of zinc phosphide three times a day,

No.	Age.	Nerves affected.	Duration of attack.	Extreme duration of treatment.	Complication.	Result.	Dose and form of remedy.
PRIMARY ACUTE CASES.							
19	M. 21	L. Trigeminal.	21 days.	5 days.	None.	Recovery	Z. P. gr. $\frac{3}{4}$ 4tis.
20	M. 32	"	48 hours.	3 "	Catarrh.	"	F. 3, gr. $\frac{1}{2}$ 3tis.
21	M. 38	R. and L. Trigeminal.	4 days.	36 hours.	None.	"	" " 4tis.
22	F. 33	R. and L. Trigeminal.	7 "	10 "	"	"	" " 3tis.
23	M. 30	L. Hemicrania.	21 "	4 days.	"	"	Z. P. gr. $\frac{3}{4}$ 4tis.
24	M. 33	R. Supra-orbital.	6 "	84 hours.	"	"	" " and F. 1.
25	M. 69	Cranial.	21 "	5 days.	Ague and Stricture.	"	F. 3, gr. $\frac{1}{2}$ 4tis.
26	M. 24	L. Trigeminal.	10 "	20 hours.	Decayed teeth (?).	"	F. 1, gr. $\frac{1}{2}$ 4tis.
27	F. 40	Cranial.	9 "	12 "	Debility.	"	F. 3, gr. $\frac{1}{2}$ 4tis.
RECURRENT ACUTE ATTACKS.							
28	F. 25	Occipito-cervical.	7 weeks.	17 days.	None.	Relief	S. H. gr. 5-10, 3tis, and F. 3.
29	F. 42	R. Trigeminal.	7 days.	12 "	"	Recovery	S. H. gr. 7 $\frac{1}{2}$ -15, and F. 2.
30	F. 36	L. "	5 weeks.	6 "	"	"	S. H. gr. 30, 4tis, and F. 2.
31	F. 38	Cranial.	2 months.	8 "	Phthisis.	"	F. 3, gr. $\frac{1}{2}$ 4tis.
32	F. 30	R. Trigeminal.	8 days.	8 "	None.	"	" " 4tis.
33	M. 28	L. "	14 "	20 hours.	"	"	F. 2, gr. $\frac{1}{2}$ 4tis.
34	M. 28	L. "	5 "	30 "	"	"	F. 3, " "
35	F. 26	R. "	28 "	4 days.	"	"	Z. P. gr. $\frac{3}{4}$ 4tis, and F. 3.
CHRONIC CASES.							
36	F. 33	Cervic-occip. R. Trig.	6 months.	5 days.	Over-lactation, &c.	Relief.	Z. P. gr. $\frac{3}{4}$ 4tis.
37	F. 20	Migraine.	5 years.	6 "	None.	"	" " "
38	F. 50	Sciatica.	Chronic.	5 "	Debility.	Recovery	F. 2, gr. $\frac{1}{2}$ 3tis.
39	M. 27	R. Trigeminal.	4 months.	3 "	"	"	F. 3, gr. $\frac{1}{2}$ 4tis.
40	F. 15	L. "	7 days.	48 hours.	Hysteria.	"	" " "
41	F. 43	Cranial and Sciatic.	21 "	" "	None.	"	" " "

* Case 33 returns in three months.

for six days, with regularity. She was then deprived of the opportunity of obtaining a fresh supply of her medicine; but she informs me, now nearly three months after treatment, that although not cured, the attacks have been much less frequent, and that whereas they formerly utterly prostrated her, she now finds them so light as not to interfere with the performance of her ordinary duties.

Case 28.—This patient had had cervico-occipital neuralgia of medium severity seven weeks. For four days she took five grains of sodium hypophosphite in water every three hours. She then thought herself a trifle better, and the dose was increased to ten grains. She continued this quantity for six days, and was then of opinion that she had derived no benefit from the treatment. She was therefore put upon F. No. 3, from which she soon received much relief. She was not a persevering patient, however, and could not be persuaded to continue to take medicine after the pain was in a measure removed.

The foregoing are the only three cases in this series not entirely successful, and each presents circumstances which, on the part of the remedy used, may be considered extenuating. I will now detail a few of the most noteworthy cases which were treated with phosphorus in other forms, first of all describing the other two cases which were in the first place submitted to the action of hypophosphite of soda.

Case 29 had suffered from severe right trigeminal neuralgia seven days, during which there had been "no" sleep. To take $7\frac{1}{2}$ grains of hypophosphite of soda every three hours. There was no improvement at all in three days, when the dose was increased to fifteen grains. As in five more days there was still no improvement, and the pain was of the severer class, the experiment was not pursued; F. No. 2 was substituted for the hypophosphite, and she recovered in four days from that time.

Case 30 had had left trigeminal neuralgia in the slighter degree five weeks; she had occasionally been deprived of sleep by it, but as a rule got worse towards and during the evening, recovered somewhat during the night, and woke with an accession of pain lasting until ten o'clock A.M. To take half a drachm of hypophosphite of soda every three hours. There was

no improvement in sixty hours ; she was therefore directed to take F. No. 2, on which she speedily recovered.

Case 25.—An old gentleman aged sixty-nine. He had been suffering for six weeks from ague, chronic bronchitis, and old-standing stricture. During three weeks he had had severe cranial neuralgia, which for five days had been excruciating, and had deprived him of sleep. In consequence he was now in a fair way to sink under his other disorders. He was put upon F. No. 3 (= gr. $\frac{1}{2}$ 4 tis. h.) After the fourth dose he had five hours' sleep, and he was entirely recovered in five days. He is the father of Case 13.

Case 27 had suffered from general cranial neuralgia (medium) for nine days. She was cured in twelve hours with F. No. 3, the mixture having been made two months previously and being the remains of that given to Case 25. (See remarks above on the stability of this prescription.)

Case 31 was suffering from her third attack. On each of two previous occasions general cranial neuralgia had followed miscarriage, attended by flooding and great exhaustion. Both trigeminal and the cervico-occipital nerves were now affected either simultaneously or in turn (severe) ; the pain had been constant with slight diurnal alleviations, and had entirely prevented sleep during the last few nights. She was in a very debilitated condition, dyspeptic, and in the first stage of consumption, of which disease her mother and sister had died. She was directed to take F. No. 3, and in eighteen hours she was very much relieved. In four days she described herself as well, although she did not lose all symptoms of nerve disorder for four days more. Under other treatment she recovered strength to a great extent, and has had no return of neuralgia (ten weeks).

Case 33 and 34 was treated for an attack of neuralgia (medium) in April last, with F. No. 2, when he recovered in twenty hours. He returned in three months, and was put upon F. No. 3 ; he recovered in thirty hours. This patient had been liable for some years to such repeated attacks. He had not therefore gained any immunity from his disease by his treatment in April ; on the other hand, the rapidity of cure on that occasion has not laid him open to a speedier return of it than was customary to him.

Case 40, a young lady, aged fifteen. Menstruation had commenced early, and she had been growing very rapidly during the past year. She had also shown during this period a want of control over the emotions, occasionally culminating in a hysteric paroxysm; and these attacks, which did not correspond with a menstrual period, were always preceded or followed by neuralgic headache and left trigeminal neuralgia (slight.) She presented the full drooping eyelid which accompanies such a condition of health, and was now suffering from all the above symptoms. She was quite cured of her neuralgia in forty-eight hours, but continued to take F. No. 3 for a fortnight. At the end of this time she was much improved in general health, and her spirits were distinctly more equable and controllable.

Case 41.—This patient has borne six children, and, as a rule, has suffered very severely during each pregnancy; but, in addition, the slightest provocation has been sufficient to excite an attack at any other time. She had now been ill more than three weeks, and at the commencement of treatment was suffering in the most severe manner. Notwithstanding the acuteness of the attack, and the frequency with which the disease has been in the habit of recurring, this patient was entirely cured in forty-eight hours, nor has she had (during one month) any return of it, except such occasional transient pains as she did not mention but on very strict inquiry.

GENERAL REMARKS.—In the number of this journal for August, Dr. Anstie records a case in which a few doses of one-thirtieth of a grain of phosphorus appear to have been followed by symptoms of poisoning. On reference to works on poisons, cases may be found in which one-eighth of a grain, one grain taken in divided doses and spread over four days, one grain and a half, and larger quantities, have proved fatal. Usually some hours elapse before well-marked symptoms occur, and several days—from five to seventeen—before the fatal result. The general symptoms are those of irritation of the intestinal tract—vomiting, purging, burning pain, and great debility; but any or all of these, except the last, may be absent, or they may all intermit, to reappear after a few hours of relief. In case of recovery they are followed by long-continued de-

bility. The special symptoms are the occurrence of collapse, or extreme weakness, without any other symptom; purpuric hæmorrhages (and from mucous membranes); jaundice; and muscular irritability or convulsions, terminating in death by coma. Or death may be quite sudden from syncope.

Post Mortem—there may, or may not, be found evidence of inflammation of the intestinal tract and perforations of the intestines: ecchymoses on the surface of various organs; softening of the muscular structures; fatty degenerations, and liquefaction of the red corpuscles of the blood.

The only cases in which symptoms approaching those of poisoning have come under my observation are Nos. 7, 35, and one other not neuralgic. In Case 7, six doses, each of one-twelfth of a grain of pure phosphorus dissolved in oil, and taken during twenty hours, were followed by violent vomiting and purging, alarming collapse, and long-continued debility. There was no hæmorrhage, however, nor any burning pain; while diarrhoea (which *was* present) seems to be one of the least constant symptoms of this form of poisoning.¹ Case 35, after six doses of two-thirds of a grain each of zinc phosphide, vomited two or three times, but took other two doses, which continued the symptom. Here again cross-examination failed to elicit any direct evidence of poisoning—no pain, hæmorrhage, sudden weakness, or purging. Lastly, the third patient, not neuralgic, took two-thirds of a grain of zinc phosphide and vomited violently; but persevered with the remedy, and took other two doses. These seemed to continue but not to aggravate the effect of the first dose. She then intermitted one day, and recovered; but, on resuming the medicine, again became sick after one more dose of it. In this case too I could not discover any special symptom of poisoning.²

The occasional existence of such a remarkable susceptibility as Dr. Anstie's case shows to be possible, should guard our administration of this drug with a caution which I confess my experience would not have led me to think necessary. That many more than the forty-one cases here recorded (in not one of which was

¹ For note of this case, vide *Practitioner*, July 1873, pp. 22, 23.

² It should be observed, however, that the phosphide was given in both these cases in the form of pill.

less than one-twentieth of a grain given in a dose, while Nos. 11 and 17 took respectively one-fifth and one-fourth of a grain every four hours for two or three consecutive days) should have yielded but one in which any dangerous condition was induced, and that not clearly traceable to the drug, has possibly hitherto been a sufficient reason for confidence. And indeed the peculiar susceptibility of this case may be nothing more than is occasionally observed in the use of arsenic, strychnia, opium, mercury, and fifty other drugs which do not require to be used with so much caution as these; ipecacuanha, quinine, or iodide of potassium, for example: and unless extended experience should reveal a greater proportion of such sensitive subjects, it will be a sufficient precaution to warn the patient to discontinue his medicine if it should seem to disagree with him. Notwithstanding this case, then, I still adhere to the suggestion that the dose of phosphorus in neuralgia should range from one-twentieth of a grain upwards; and the further experience now recorded does not lead me to alter the opinion that a *less* dose than this is insufficient to secure the success which may confidently be looked for from its use in this disease. A smaller dose, no doubt, well answers the purpose of a special tonic in most other cases.

In the course of the experiments of which the result as regards neuralgia alone is here published, I have observed cases of other diseases in which I believe further experience will show phosphorus, either pure or in one of the forms described above, to be a remedy most valuable and of somewhat wide applicability. For neuralgia it appears to me to be a remedy as well deserving to be called specific as are mercury and quinine for syphilis and ague. Still, as in these diseases cases occur which seem to be beyond the reach of these drugs, so cases of neuralgia must be expected to occur which refuse to yield to phosphorus. The sphere of usefulness of the pure drug has thus far seemed to be almost limited to this form of nerve disorder (including migraine) and to the special debility of impotence. I have lately treated two cases of the latter disease, in which it was difficult to assign a cause for it. They both occurred in young men between thirty and thirty-five years of age; one had been long married—the other was about to be;

and both had been under general treatment during three or four months (nearly the whole period of illness) without deriving the least apparent benefit. Doses of one-fiftieth of a grain of strychnia, with one-twentieth of a grain of phosphorus, soon relieved both cases—the latter proceeding to fulfil his engagement.

I have a few cases of epilepsy under treatment with phosphorus in one form or another, and I believe I have observed all of them to be benefited by it. In one or two the result has been remarkable. I will not further enter upon these cases here than to remark that the result in one of them seems to be precisely analogous to that observed in Case 40 and some other hysteric females—I mean a marked increase of power of self-control. The case alluded to was one of very old standing, and the patient was now subject to fits of uncontrollable passion upon little or no provocation. This state was very much aggravated at the end of each lunar month. The result of two or three weeks' treatment with phosphide of zinc was a remarkable decrease of irritability. Although still *liable* to be excited by the merest trifle, he was now amenable to an admonition or an explanation, which would formerly have only aggravated the supposed offence; and his friends exhorted him to control himself in a way they had not ventured upon for many years, and with a result as satisfactory as it was surprising to them. So Case 40 was reduced from an excitable nervous condition to one of comparative calmness; and in this case, too, although she still *inclined* to allow the predominant emotion to get the better of her good sense, a hint or a look judiciously given was now quite enough to enable her to regain her equanimity. I thus briefly compare these two cases here since they seem to have something in common in the submission of a marked condition of both to the same remedy; and they may perhaps tend to throw some light upon the mode of action of it. The expression "recovery of self-control" may perhaps be taken exception to; it is only intended to describe practically one result of the treatment.

In cases of obstinate leucorrhœa, assigned as one of the factors of neuralgia, local or general, by Dr. Anstie, I have observed marked benefit ensue upon the use of hypophosphite of soda

in the smaller doses—five or ten grains three times a day—although not accompanied by any more definite neuralgic symptom than the side-ache so frequently complained of by these patients. A striking improvement in respect of this pain, as well as in the amount of the discharge, has been noted in cases which had resisted a long and varied course of local and general tonic treatment.

In the notices which have lately begun to advertise preparations of phosphorus, many and somewhat various diseases are alleged to be amenable to its influence. Chiefly they are diseases accompanied by or apparently the result of exhaustion. I have given pure phosphorus in some of these; in diarrhoea, for instance, both of adults and children—or rather in the consequent debility; and on several occasions in convalescence from typhoid fever. In none of these cases have I observed any remarkably rapid improvement; and in its action on the latter condition nothing has been noted to justify its substitution for quinine and the mineral acids. But it seems not at all improbable that one or other of the compounds of phosphorus may have a somewhat different scope of action from the pure drug. Some cases of dyspepsia have been strikingly relieved by the addition of hypophosphite of soda to the prescription which had been used for some time with no particular advantage, and that in cases in which F. No. 3 had been given in conjunction with but not in combination with the other remedies. And in some of the cases of epilepsy, phosphide of zinc has seemed to have a beneficial effect when hypophosphite of soda and pure phosphorus have both failed to make any impression. Such cases as these, however, require a much more extended observation and experience than those of neuralgia, since the result of treatment cannot be so obvious or so well marked. They may perhaps be considered with advantage at a future time.

Reviews.

The West Riding Lunatic Asylum Reports. Edited by G. J. CRICHTON BROWNE, M.D., F.R.S.E. Vol. III. London: Smith and Elder, 1873.

DR. CRICHTON BROWNE may be complimented on the very valuable collection of original papers which he has published in the present volume of the West Riding Lunatic Asylum Reports. There are fourteen of these papers, and we are not expressing ourselves too strongly when we say that nearly all of them deserve very careful study; some for their original research, and others for their importance in a practical point of view. If Dr. Browne can obtain for his next few numbers as valuable a series of contributions as are contained in the present one, the West Riding Lunatic Asylum Reports will become one of the most important medical periodicals of the day. The central essay, to which most interest attaches, is undoubtedly Dr. Ferrier's, containing an account of the researches, of which an abstract has already appeared in the *British Medical Journal*, on the functions of the cerebrum, determined by experimental investigation. Hitherto the investigation of the brain by this method of research has led to exceedingly poor and unsatisfactory results; in fact, the results that have been obtained have been chiefly negative. It has been long known that removal of large slices of the brain is attended, even in the human subject, with very little, if any, pain, and that the subsequent effects are by no means commensurate with the supposed importance of the cerebral convolutions in psychical operations. The recent progress of clinical research has, however, pointed out to the physician that, as in the case of aphasic patients and the loss of muscular co-ordination in cerebellar mischief, lesions of certain parts of the brain may be attended with very definite symptoms, and that a fairly accurate diagnosis can often be made by careful attention to the symptoms.

The experimental physiologist has now, in his turn, taken up the subject, and the results obtained are certainly not only novel, but likely to throw a large amount of light on various points in the obscure domain of cerebral pathology. The lately published

experiments of Fritsch and Hitzig have, for example, shown that certain movements of the eyes and limbs can be induced by the transmission of electrical currents in definite directions through the brain; and still more recently Nothnagel has investigated with some success the action of small quantities of chromic acid injected into different parts. The effect of this proceeding is to kill the part injected, whilst it has the advantages of its action being limited to a given spot, which may further, by its colour, be readily removed after death, and subjected to microscopic research, whilst the operation of injection itself causes little injury to the general well-being of the animal.

Dr. Ferrier's experiments have been performed on dogs, cats, and rabbits, and the general results obtained have been remarkably uniform and mutually corroborative. Some experiments made on birds proved less satisfactory, and the relations and homologies of the parts in these animals require further investigation. The plan adopted was to expose a small region of the brain by the use of the trephine, then to cut away the dura mater, which is exceedingly sensitive, and to apply a faradaic interrupted current to the part exposed. It was found that stimulation of the surface of the hemispheres by this means causes hyperæmia of the parts irritated, leading, even when every care is taken, to considerable hæmorrhage, through which the functional activity of the part is speedily depressed or entirely abolished. It was further demonstrated that the anterior portions of the cerebral hemispheres are the chief centres of voluntary motion and the active outward manifestation of intelligence. In these parts are located special centres, on stimulating which, movements can be called forth in the eyelids, in the face, in the mouth and tongue, in the ear, neck, hand, foot, and tail. The action is in general crossed, but this crossed action only extends to muscles and combined muscular movements which act independently of one another. Irritation of the posterior lobes of the brain, both on their external surface and on the internal or hippocampal surface, as well as irritation of the gyrus fornicatus, always failed to produce outward manifestations: and these regions are probably connected with sensory tracts, as are also the optic thalamus, fornix, and hippocampus major. On the contrary, the corpora striata have crossed action and are great motor centres. Powerful irritation of one causes rigid pleurosthotonos, the flexors predominating over the extensors. The optic lobes, or corpora quadrigemina, besides being concerned with vision and the movements of the iris, are centres for the extensors of the head, trunk, and legs. Irritation of these centres causes rigid episthotonos. The cerebellum is the co-ordinating centre for the muscles of the eyeball, and on their integrity depends the maintenance of the equilibrium of the body.

It is not only, however, in thus confirming and extending our knowledge of the functions of the different parts of the encephalon that the experiments of Dr. Ferrier are of value, but they also support in a very remarkable manner the conclusions of Dr. Hughlings Jackson on the pathology of chorea, epileptiform convulsions, and kindred affections; for they show that whilst moderate stimulation of a centre causes merely the apparently normal excitation of the muscles co-ordinated there, more powerful stimulation excites an epileptiform condition of the same muscles; while diffused irritation of the whole hemisphere, by causing the current to travel from one end to the other, is sufficient to produce general convulsions, usually, but not always, restricted to the opposite side. Hence we see that in accordance with Dr. Jackson's hypothesis, the disorderly, yet to some extent co-ordinated movements of chorea, may be regarded as being caused by slight discharges or explosions of unstable grey matter in a definite part of the cerebral convolutions; whilst epileptiform seizures, varying from the slightest attack of the "petit mal" to the most violent general convulsions and tetanus, are explicable on the same view, the latter being the result of more violent explosions. And it is a singular circumstance in Dr. Ferrier's researches, that when a portion of the hemisphere or convolutions has been frequently artificially excited, an epileptic habit is, as it were, established in the brain generally, which leads to the occurrence of fits on such slight irritation that in his experiments the localised exploration of the individual motor centres had sometimes to be given up.

There remains one point more to notice, namely, that these experiments show the importance, for the purposes of diagnosis and exact localisation of the seat of lesion, of paying particular attention to the march of the spasms, and the order in which the convulsions begin and terminate. By such attention it appears to be possible to fix, in many instances, the precise position of the cerebral lesion to which the fits are owing; and thus an immense stride in the knowledge of the etiology of certain forms of disease will be gained.

We have noticed this paper at such length that there is little space to allude to several others in the same volume of great interest and importance. Amongst these are two by Dr. Hughlings Jackson, entitled "Observations on the Localisation of Movements in the Cerebral Hemispheres," and "On the Anatomical, Physiological, and Pathological Investigation of Epilepsies," which, though well worthy of comment, we pass over, as the general tenor of his remarks is already familiar to those who have read his lectures in the *Medical Gazette* and elsewhere.

Dr. Major contributes some observations on the "Histology of the Brain in the Insane," in which he gives the result of his

microscopical examination of those who have died from chronic brain-wasting, senile atrophy, and general paralysis. In all he finds well-marked evidence of disease, such as dilatation of the vessels, atrophy and pigmentary degeneration of the cells, patches of molecular degeneration in the white substance; but none of these changes appear to be absolutely limited to or characteristic of any of these forms of disease.

The volume contains, finally, a group of papers which open up some interesting lines of inquiry. Thus Dr. Fothergill and Dr. Burman both discuss the relations of heart-disease to insanity; Mr. Galton writes on the condition of the tympanic membrane in the insane; Mr. Clapham, on the weight of the brain in the insane; and Dr. Sutherland on the relations of change of life and insanity—all papers of much interest and sterling value.

Diseases of the Ovaries: their Diagnosis and Treatment. By T. SPENCER WELLS. London: Churchill, 1872.

Ovarian Tumours: their Pathology, Diagnosis, and Treatment, especially by Ovariectomy. By E. RANDOLPH PEASLEE, M.D., &c. New York: Appleton, 1872.

THE history of the operation of ovariectomy is one of the most instructive in the whole range of surgery. Originally performed under very creditable circumstances by Dr. McDowell, of Kentucky, in 1809, and repeated many times successfully both in America and in England, it was violently opposed by Liston, Mütter, Duncan, and Lee. The vigour of the attack made by the last of these authors was so great, that even the philosophical mind of Lawrence was swayed from its equilibrium; and though he admitted that he had never seen or performed the operation, he declared that unless he greatly changed his opinion he never would undertake it; and this notwithstanding that Hunter had long before remarked that hydatids of the ovary might be taken out, and that "there was no reason why women should not bear spaying as well as other animals." The suffering and inconvenience caused by ovarian dropsy, its commonly fatal termination, and the advance of conservative surgery generally, nevertheless still continued to lead various surgeons, who were desirous only of relieving their patients from a burden which threatens existence itself, and who were untrammelled by the dicta of authorities and schools, to the performance of the operation; and the successful issue of numerous isolated cases at length induced Mr. Spencer Wells to try the operation systematically. His success was so considerable as to lead to its complete revival, and his example has been followed by numerous surgeons both in this country and abroad; and though many fatal cases occurred amongst the earlier patients operated upon, the fact that considerably more than a thousand women have submitted

to the operation, whilst the mortality has steadily diminished as the diagnosis has become more satisfactorily determined, and as the details of the operation have been more and more attended to, till it is now no greater than such an operation as amputation at the shoulder,—is sufficient evidence that it is now one of the established operations of surgery.

The two large and important works before us are so similar in their scope that there will be some advantage in considering them together. Both commence with an anatomical and physiological description of the ovary, Mr. Wells giving in addition a short account of the reproductive apparatus generally, but this chapter may be passed over in silence, since in neither is there any pretension to original research. The classification of ovarian tumours succeeds. Dr. Peaslee divides them into *solid tumours*, including enchondroma, osteoma, carcinoma, papilloma, fibroma, which last may affect either the corpora lutea or the stroma; and *cystic tumours*, including hydrops folliculorum, cystoma ovarii, which he divides into struma, oligo-cysts and polycysts, and dermoid cysts. Mr. Wells makes a primary division into ovarian and extra-ovarian tumours. The ovarian he divides into *adenoid*, including simple, multiple, and proliferous cysts; *fibrous*, and *malignant*, including cancer and tubercle. The extra-ovarian he divides into *cysts of the Fallopian tube*, *cysts of broad ligament*, or vesicles of Wolffian body, *cysts developed from tubules of parovarium*, and *cysts developed from aberrant ova* attached to the peritoneal surface. Dr. Peaslee has a chapter on the position and relations of ovarian cysts. He admits four stages of growth. In the first, the cyst is still within the pelvis; in the second, the upper part of the tumour is extending towards the umbilicus; in the third, it has advanced beyond the umbilicus; and in the fourth and last stage, it increases in girth alone, having previously reached its highest point. A drawing illustrates these conditions. The character of the contents of the cysts is given at greater length by Mr. Wells than by Dr. Peaslee, who appears to have overlooked Eichwald's valuable paper in the *Wurzburg Zeitschrift*.

In regard to the origin of ovarian cysts, Mr. Wells cites with approval Dr. Ritchie's view, that many cases are actually due to degeneration of the ovum itself, the cyst being formed by an enlargement and thickening of the blastodermic vesicle—a view that does not seem in itself improbable. Dr. Wilson Fox, however, who has bestowed much attention upon the question, is disposed to think all the forms of cysts met with in the ovary originate from the Graafian follicles, the multilocular forms being due to secondary formations from the interior of parent cysts. Mr. Wells himself admits that the cysts may take origin quite independently of the Graafian follicles, from the deep

areolar tissue, or among the vessels of the gland. Peaslee, in accordance with Rindfleisch, Kiwisch, and others, is fully of opinion that the colloid cysts at all events do not arise from Graafian follicles.

The difficulties of the diagnosis of ovarian disease are well known, but since the time when Lizars and Dieffenbach opened the abdomen of their patients and found no tumour of any kind, immense advances have been made; and in view of the elaborate account of the differential diagnosis given by both our authors, few, if any, mistakes should now occur. In fact, Dr. Keith, of Edinburgh, could say two years ago that he had performed ovariectomy 136 times without an error in diagnosis. Dr. Peaslee enumerates no less than twenty-four distinct diseases that may be or actually have been mistaken for ovarian tumour; and Mr. Wells gives about the same number, each mentioning one or two not given by the other. Some of these, however, could only be mistaken for ovarian disease from gross carelessness. The diagnosis of ovarian dropsy from ascites is given at great length by both authors; and Dr. Peaslee mentions a suggestion made by Dr. Atlee, that may in some instances be of importance, and is at least novel. It is that the pulsations of the aorta are not felt by placing the hand over the anterior part of the abdomen in ascites, whilst they are transmitted to this surface through an ovarian tumour.

The details of the treatment of ovarian cysts, of course, constitute the most important and most carefully considered section of these works. Ovarian cysts admit properly of surgical treatment alone, for it is now generally conceded that medical treatment is either of negative value or has only proved successful in some exceptional cases. The surgical treatment may be divided into the palliative treatment by tapping; curative treatment by tapping and pressure, or tapping and injection, or by the formation of a permanent fistulous opening; or finally, by ovariectomy. In regard to simple tapping, it appears to be attended with greater danger than might at first sight be supposed; Dr. Meigs, of Philadelphia, stating that nearly one-half of the first tapplings of ovarian tumours which he had witnessed had proved fatal; whilst Dr. Storer even gives, after Stilling, as the first of his ten golden rules of ovariectomy, "Never tap an ovarian cyst."

Mr. Wells precludes his account of ovariectomy by a short chapter upon its history, in which, after referring to some of the ancient writers and doubtful cases, he observes that no one can dispute the validity of the direct claim of McDowell (of Kentucky) as designedly the first rational ovariectomist. He then gives the cases of Houstoun, Lizars, Granville, Jeaffreson, King, West, Benjamin Phillips, Caesar Hawkins, and others, till he

arrives at his own very large series, and those of Dr. Keith, of Edinburgh. A few pages are devoted to the history of the operation in Belgium, France, Germany, and Russia. This part of the subject is treated at much greater length by Dr. Peaslee, who has really exhausted it, not only so far as regards the United States, but in reference to every European country. It is satisfactory to find that by far the largest average of successful cases occurs in English and American practice; Dr. Keith having saved eighty-one patients out of his first hundred cases, and thirty of his next thirty-six; whilst Mr. Wells saved 73 $\frac{1}{2}$ per cent. out of his first four hundred cases, and between April 1870 and March 1871 he had thirty-two successive cases of ovariectomy in private practice without one death. The Americans have also been extremely successful, Atlee having cured 70 per cent. in 246 cases, Kimball 66 per cent. in 121 cases, Dunlop 80 per cent. in sixty cases, Peaslee 68 per cent. in twenty-eight cases, and Bradford 90 per cent. in thirty cases.

An extremely interesting fact is deducible from the statistics given by Mr. Wells—that, namely, on the whole series of 500 cases there is only a difference of little more than 2 per cent. in favour of cases in private practice as compared with those treated in hospital, showing clearly how much may be done when great care and pains are taken, in reducing the mortality of serious operations, even under what are commonly regarded as unfavourable conditions. With all this, however, Mr. Wells, strongly recommends patients to be, if possible, operated on in the country, and in their own houses, where he thinks the mortality may yet be considerably diminished. The far lower average of success obtained by operators in France and Germany is partly due, there can be little doubt, to want of care in the minor details of the operation, and especially to a want of cleanliness, which the judicious observations of Billroth, Koeberlé, and others will do much to improve; and partly to the operation being deferred, from the fear of consequences, to too late a period. In regard to preparatory treatment, Mr. Wells insists strongly on the importance of correcting that condition in which only a small quantity of highly concentrated urine is deposited, since, if this be neglected, uræmic febrile symptoms are almost sure to occur; for this purpose he admits tapping may be requisite as a preliminary measure, with the free use of warm baths, diluent and alkaline carbonated drinks, as of lithia alone, or lithia, potash, and soda combined, with or without perchloride of iron, the value of which was much insisted on by Simpson. Mr. Wells gives some really admirable observations on nursing and the choice of nurses, which ought to be read by every lady who imagines herself qualified to undertake these duties, and might perhaps deter some from entering a

career for which they are not adapted. As an anæsthetic, Mr. Wells prefers the bichloride of methylene to all others, producing, when employed in Junker's apparatus, calm, quiet sleep, with no tendency to subsequent sickness. Dr. Peaslee, in common with most American operators, prefers ether, not pushed to deep narcosis, but simply to anæsthesia. Both authors agree that the minor operation, in which the wound does not exceed from three to five inches, is the safest; but when adhesions are extensive, it is better to make a free opening than to run the risk of exciting inflammation by the employment of force. Dr. Peaslee says, very significantly: "I have seen an operator tug for fifteen minutes at a collapsed cyst, attempting to draw it through an incision two inches long, and then desist from the operation because 'there were strong and universal adhesions.' The post-mortem, three days after, showed there were no adhesions at all, and that the operation would have been one of the simplest, and probably also the most satisfactory, with an incision of even three inches." The median line is probably the best position for the incision to be made, though Atlee and others have made lateral or even transverse incisions successfully. Mr. Wells holds the clamp, which of course dispenses with the necessity for any ligatures, in very high estimation, and sees no reason for changing the opinions in its favour he expressed several years ago in the *Medical Times and Gazette*. When, however, from the shortness of the pedicle, the traction on the uterus or broad ligament would be painful or dangerous, he either ties the pedicle lightly with a fine silk ligature, or applies the actual cautery and allows the parts to recede. It is not difficult to understand the secret of Mr. Wells's great success in the operation on reading this part of his work. It is evidently due to the great care and pains taken in every step. If possible he allows no drop of the ovarian fluid to enter the peritoneal cavity; vessels are tied as they are divided; and towards the close of the operation the peritoneal cavity is thoroughly cleansed from any fluid or clot which it may contain. He observes that he has often regretted incomplete sponging—never that he had been too careful: and he finds it very convenient to insert a large, broad, flat piece of thin sponge just within the wound, and leave it all the time that the sutures are being passed. It protects the intestines and peritoneal cavity generally, and catches any drops of blood which may follow the passage of the needles. For the closure of the wound he finds sutures made of thin, strong, Chinese silk superior to hemp, twine, silk, silver and steel wire, telegraph wire coated with gutta-percha, or strong horsehair. So also, in the after treatment, it is obvious that the woman's safety depends upon that attention to a multitude of small details which constant practice can alone

give, and of which there is no better proof than that with all operators the ratio of successful cases notably rises with increasing numbers.

The literature of the treatment of the pedicle is exhausted by Dr. Peaslee. He gives the special mode adopted by no less than twenty-three different operators, with the reference to each case. He considers that the various methods may practically be reduced to two, the clamp and the ligature, and apparently gives the preference to the latter. He says little of the method of cauterising and returning the whole into the abdomen, though we believe this is the plan now pursued by the younger portion of the staff of the Women's Hospital, Soho. In regard to the subsequent treatment, after discussing the dangers of hæmorrhage, vomiting, tympanitis, &c., he proceeds to consider septicæmia. For the treatment of this state he proposes intraperitoneal injections, and gives a series of cases where the patients were clearly saved by their use; and he now endeavours to prevent septicæmia by the introduction of a tent into the lower part of the wound. If all goes well, this is removed on the fourth day, and the wound is closed; otherwise the intraperitoneal plan of injection is practised. Mr. Wells has tried this plan in a few bad cases, but with no success. Dr. Peaslee scarcely does more than allude to the plan of introducing a drainage tube through the walls of the vagina and the fold of Douglas, but Mr. Wells states that he has had one successful case.

Both authors give numerous drawings of instruments, and of the various steps in the operation; and Mr. Wells, in addition, gives a specimen of his "Form of Note-book for taking cases," which is singularly complete.

Upon the whole these volumes are highly creditable to the authors. They are both written very carefully by men who are thoroughly familiar with the diseases they describe. Dr. Peaslee's may be regarded as the most learned work of the two, containing a larger amount of research and more numerous and exact references; whilst Mr. Wells is somewhat more practical, and rather pleasanter reading, in consequence of its being less broken up; but either may be taken by the young practitioner as containing all particulars that may guide him to the right selection of cases for operation, and to the right mode of operating and subsequently treating the patient in any given case.

Clinic of the Month.

Enlargement of Bronchial Glands.—Dr. Gueneau de Mussy, in a paper read before the section of Medicine at the late meeting of the British Medical Association, observed that the enlargement of the bronchial glands, noticed by the anatomists as very common, has been described by the pathologist only in its most severe forms, and is very rarely met with in adults. It is, however, he thinks, very common. It may complicate all the affections in which the respiratory organs are concerned, and modify both the physical and the physiological symptoms of these affections. It usually produces cough or dyspnoea, in some cases aphonia and vomiting, according to the relation of the enlarged glands to the pneumogastric nerve to a portion of that nerve. Protracted hooping-cough, lasting, it may be, some years, is connected with this enlargement. The physical sounds are rubbing, impairment of elasticity, and acute percussion sound at the upper part of the sternum, the inner part of the first two ribs, the intercostal spaces, and the sterno-clavicular joint; and posteriorly over the laminae of the first four vertebrae, usually on one side. On auscultation there are weakness, acuteness, roughness of the respiratory murmur in one part or in the whole of one lung; generally protracted respiration, sometimes localised sibilant rhonchus; and very often near to the spine and to the sternum, an expiratory *souffle*, which is the tracheal respiration conducted by the enlarged glands. Sometimes these sounds may be modified by the movements of the neck. The treatment recommended by Dr. Gueneau de Mussy was iodine internally, and locally chloride of sodium, arseniate and carbonate of soda, and such general means as improve the lymphatic constitution. (*British Medical Journal*, Aug. 30, 1873.)

Intravaginal Auscultation in the Diagnosis of Pregnancy.—At the Academy of Medicine of Bologna, Dr. Verardini lately (*Gaz. Med. Ital.*) read a paper concluding—1. Internal or intravaginal auscultation is of the greatest importance for detecting pregnancy during the first months. A characteristic bruit may

then be heard, if the instrument be pressed against the cervix uteri. 2. This method was first announced by Meygrier, in 1825, and four years later was employed by Nauke, but latterly has been brought into frequent use, in England, by Routh, who, in conjunction with the author, found that he was possessed of the means of recognising both early pregnancy and placenta prævia. 3. If it be impossible to make a satisfactory diagnosis of placenta prævia during the early months of pregnancy, it can always be determined during the later stages, from the continuous bruit. 4. The utero-placental bruit, the indication that the foetus is pressing against the uterus, is soft and prolonged, similar to that heard in aneurismal tumours when the stethoscope is pressed upon arteries. When once heard it can always be detected a second time. 5. It is important always to be sure that there is no pulsating tumour or artery in the neighbourhood of the cervix. 6. The prolonged utero-placental bruit, which is distinctly appreciated during the first months, ceases at the commencement of the sixth or seventh month. 7. If other symptoms of pregnancy are present, and this characteristic is absent, the diagnosis will remain in doubt. It is, then, most probable that there is uterine disease; certainly, if there have been symptoms that pregnancy has at some time taken place, there must have been disease of the ovum, followed by death of the embryo or foetus. 8. In making the examination the patient may lie upon the back or the side; but if in this position the accoucheur fails to hear any bruit, then let him place the patient in the knee-elbow position, and he will succeed without difficulty. 9. Internal auscultation is of great importance in practical and forensic medicine and in surgery. Early pregnancy can thus be diagnosticated, and many possible errors avoided. 10. The name given to the instrument employed is the vagina-uteroscope. It can be made of various shapes, and is usually of gutta-percha, to render it as light as possible. (*Medical Press and Circular*, Aug. 6, 1873.)

Extracts from British and Foreign Journals.

The Treatment of Tetanus.—Dr. B. Roemer, after reviewing the various modes of treatment adopted in reported successful cases of tetanus, sums up his experience as follows.

1. Division of a nerve can be of benefit only if practised very early; amputation is similarly practicable, but generally of little avail, because decided symptoms have already been developed.

2. Local applications and general attention to the wound are of the utmost importance to prevent complications, and to ensure local quietude and avoid derived irritation. Fresh air is for a like reason instrumental in the successful management of tetanus.

3. General and unconditional quiet and rest around the patient are enjoined; all noises, even to speaking in an elevated tone, are to be interdicted, remembering that sudden death has often followed after exciting impressions.

4. Supporting nourishment in concentrated form, and stimulants, should supply the exhaustion in a disease which is not characterised by disorganisation of any vital organ.

5. In cases where the patient is plethoric, the pulse full but not frequent, and where there exists a disposition to, or symptoms of, isochronic inflammatory action, venesection, leeching, and cupping are indicated in the outset, and should be promptly but carefully practised. The subsequent action of the therapeutic agent is thereby rendered more certain.

6. Purgatives of prompt action are necessary in all instances, and should be repeated and interchanged until a desired effect is obtained, aided by enemata if demanded.

7. The selection of the anti-tetanic should be made according to the most promising statistics on tetanus, and with these references:—

(a) As corroval and its kindred remedies act upon the heart by paralysing its muscular action, the cardiac condition should be carefully diagnosticated, its normal status ascertained by commemorative evidence and symptoms—the tendency of the

spasm, flushed face, etc., with the general phenomena of functional or organic disease—and the effect of the remedy, especially in cases in which no morbid action of the heart was found, but in which medication tends towards interfering with the cardiac action, should be considered.

(b) When the remedy in a quick but cautious exhibition sustains itself by the systemic tolerance, then the same should be continued in adequate doses until its full therapeutic virtues are elicited.

(c) The remedy should be given in a form easiest taken up in the system, to ensure its action as soon as possible, to avoid accumulative effects, and with due regard to the difficulties of deglutition.

(d) It should be continued in its full effects upon the system until the abnormal condition of the spinal cord and nerves has been exhausted or annihilated.

8. As standard remedies, opium, cannabis indica, and the Calabar bean are entitled to the greatest confidence (in their purity of preparation), and their mode of exhibition is exemplified in various of the cases reported on. (*Saint Louis Medical and Surgical Journal*, July 1873.)

Sulphate of Quinine in Optic Neuritis.—M. Deneffe, accepting the views recently advanced, that sulphate of quinine excites the vaso-motor nerves, and consequently causes contraction of the blood-vessels, recommends its use in cases of retinitis. In one case in which he tried it, diarrhoea set in, which, however, was successfully combated by decoction of rhatany, with the addition of a little watery extract of opium. After the diarrhoea was arrested, improvement in the power of vision commenced, so that in the course of a month the patient, who was suffering from an attack of idiopathic retinitis, was regarded as almost completely cured. (*Centralblatt*, 1873, p. 255.)

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¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

Department of Public Health.

THE ABATEMENT OF EXCREMENT NUISANCES.

FOREMOST among the questions which have to be dealt with by the newly created Sanitary Authorities in the kingdom, and by their officers, is that of the prevention of excrement nuisances. We use the unvarnished term lately adopted for this kind of nuisances by the Medical Officer of the Local Government Board, Mr. John Simon, not merely on the principle of calling a spade a spade, but chiefly because the euphonistic terms most commonly in use to designate the subject, namely, "The sewage question," "The treatment and utilisation of sewage," "The removal of excreta," have a tendency to divert the mind from the essential considerations which it is incumbent upon Sanitary Authorities and their officers first to have regard to, namely, health considerations. Indeed, matters have pretty well come to this pass, that, paradoxical though it may seem, the prevention of excrement nuisances, as a question of health, has become subsidiary to the consideration of dealing with them as a question of means. Sanitary Authorities, bewildered with the many plans offered for their guidance, and tantalised by hopes of the discovery of a royal and universal method suggested by enthusiastic sanitary inventors, have in the greatest number of instances suspended all action, and the nuisances they were bound to prevent have remained unabated. The practicable has in fact been sacrificed to the possible.

The operation of the Public Health Act, 1872, will for the most part bring this inaction of Sanitary Authorities to an end, and they will be compelled to adopt some measures for the abatement of excrement nuisances within their districts. It is of the

greatest moment, therefore, that they and their officers should understand clearly in what manner the prevention of excrement nuisances may be accomplished, so that they may apply this knowledge to the particular requirements of their respective districts. The principles which should guide the action of a Sanitary Authority in this matter have been fully and authoritatively set forth in an official paper published by the Medical Department of the Local Government Board, entitled "*Report on certain means of preventing Excrement Nuisances in Towns and Villages.*" This report is based upon a detailed examination of the different arrangements in use for the disposal of excrement in numerous towns of England and Scotland, particularly the arrangements for the wage-earning and poorer classes, who are and must be the most dependent upon public arrangements and public supervision. A summary of the arrangements amongst these classes is given as follows :—

A. THE MIDDEN SYSTEM :—

- a. *Of old type, old parts of almost all towns.*
- b. *Middens of large size, and permitting much accumulation, but compulsorily supplied with some means for keeping the contents dry (covers, drains, or both), and for preventing leakage into the earth. Preston, Leeds, Birmingham.*
- c. *The same (though generally with smaller middens) with the addition of special construction aiming at the effectual covering of excrement by ashes—*
By sloping bottom. Nottingham, Stamford.
By hinged seats or steps. Manchester, Salford, Saltaire, Stamford.
By ash-bin and shoot. Manchester.
- d. *The same arrangements, but with the midden reduced to a mere space under the seat. Hull.*

B. PAIL OR TROUGH SYSTEMS, with cartage :—

- a. *Boxes used without preparation. Leeds.*
- b. *Boxes supplied with a little earth. Nottingham.*
- c. *Tubs supplied with a little salt and fine ash. Rochdale.*
- d. *Tubs lined on the Goux system. Salford.*

- e. *Pails into which ashes as well as excrement are thrown, and deposited daily in the streets.* Edinburgh.
- f. *Troughs or pans used with or without a little water.* Glasgow, Edinburgh.

C. THE EARTH SYSTEM. Lancaster.

D. THE WATER SYSTEM, *extended with special appliances to public conveniences and the poorer districts:*

The trough water-closet. Liverpool.

The tumbler water-closet. Leeds.

The conclusions of the reporters (Dr. Buchanan and Mr. Netten Radcliffe) on the different systems of excrement disposal which came under their observation are so full of instruction that we quote them without curtailment, italicising the portions to which we would direct especial attention.

"In the present imperfect state of our knowledge of the conditions under which *fecal* diseases spread, we do not feel ourselves entitled to say at what time, after being passed, dejections are or may (under various external circumstances) become dangerous to health. We cannot say this either in regard of healthy excrement, or of that passed from persons affected with disease, specific or other; but we think it may probably be taken as sufficiently true for practical purposes that there is little chance of mischief from the storage of excrement *for a day*, even though along with healthy excrement that of persons affected, for example, by enteric fever, should, without proper disinfection, chance occasionally to be included. We propose, then, to regard *complete removal of all excrement within a day* as practically constituting safety in the case where excrement is unmixed, or is only mixed with ashes.

"The general conclusions which we would submit as resulting from our inquiry are as follows:—

"1. Excrement may be removed from a town and disposed of on more than one principle, and the same principle does not need to be applied in all quarters of the same town.

"2. As regards the part of a town inhabited by the poorer classes, a water-closet system may be managed so as to be entirely applicable to the circumstances of the most ignorant and most careless population. *Essential conditions of such applicability, however, are that the structural arrangements should be adapted to their purpose and be independent of the person using the closet, and that the management should be wholly undertaken and efficiently done by the servants of the sanitary authority.* Where these conditions are observed as thoroughly as they are observed in parts of Liverpool, we believe that water-closets are the best means of removing excremental matters from the poor neighbourhoods of a town.

"3. The earth system affords a second way of safely disposing of excrement. It is, as shown in the special report on earth-closets, *an essential element in this system also, as applied in poor neighbourhoods, that the entire management of it shall be conducted by the sanitary authority.*

"4. The midden system may be modified so as greatly to reduce nuisance and danger from it. We have described the form of midden-closet which we think presents fewest objections. We cannot speak of satisfactory safety in the use of even this form of midden-closet, partly because we hardly expect to see it carried out with daily emptying, and partly because the materials of the midden would probably be retentive of some excremental matters; but *if, under certain circumstances, middens constructed as above should be tolerated, it would, we think, be scarcely less than essential, first, that they should, if in a densely populated neighbourhood, be emptied daily, or under other circumstances at least once a week; and secondly, that the arrangements for excrement removal should be wholly in the hands of efficient persons appointed by the sanitary authority.*

"5. The pail system presents several advantages for poor town districts. It may safely be employed for excrement removal if moveable pails of defined construction be used, and be changed every day for fresh pails. Such a system, involving construction or constructive alterations similar to those required for the toleration of a midden system, offers advantages over the latter in regard of facility for frequent removal of excrement, in regard of safety from nuisance, and probably in regard of profit in disposing of excrement as manure.

"6. Those who use the closet may, both under the pail and the midden system, be expected, with due superintendence, to do the cleansing of it so far as merely affects ordinary comfort and decency; but *such action as concerns the effectiveness of the closet as a means of excrement removal must be taken by the sanitary authority itself.*

"7. If these conclusions be accepted, it follows that there are various demonstrable methods which will fairly answer the purpose of preventing nuisance and injury to health from the retention of excrement, at least until the perfection of arrangements for dealing with excrement shall be agreed upon. It cannot yet be affirmed of any one of the methods that it will develop into the only perfect system of the future."

It would appear, then, from the reporters, that of the different arrangements now in use in England for the disposal of excrement, all may be carried out so as to reduce nuisance from this source within very narrow limits. The importance of this conclusion, particularly in respect to small towns and villages, cannot be over-estimated. The great stumbling-block in the way of extensive information in the arrangements for excrement disposal in this country during the last quarter of a century has been the resistance of sanitary engineers, practically of engineers having an official position, in advocating a water-closet system to the exclusion of all other systems. This advocacy has been maintained irrespective of the consideration that such a system could only be very partially applied in our towns and villages, from the difficulty, and in some instances impossibility, of obtaining a sufficient water supply. The late General Board of Health unfortunately gave the weight of its authority to this

one-sided advocacy, and it was not until the formation of the Medical Department of the Privy Council, under Mr. John Simon, that the state of towns and villages which either refused to adopt a water-closet system, or in which such a system could not be carried out, received systematic official attention. The Report before us proves conclusively that a Sanitary Authority in dealing with the disposal of excrement need not restrict its choice to any one known system, and that more than one system may often very usefully be used in the same town or village. It proves also that whatever system may be adopted, it is quite practicable to carry out this system without the production of nuisance. The importance of this latter conclusion in respect to a midden system deserves particular consideration. It must be taken as a fact that for many years to come the midden-closet will still be the principal mode of excrement disposal in many towns and villages. Hitherto this mode has, as a rule, been carried out in the most barbarous and noxious fashion, partly from an ignorant prejudice, namely, from the belief that it could not be amended otherwise than by the adoption of a water-closet system. The experience of Hull has now conclusively shown that a midden system can be so arranged as to remove altogether its most obvious offensiveness and nuisance; and the experience of Manchester and Salford shows that even midden-closets with pits of large capacity can be so constructed and managed as very considerably to diminish their unwholesomeness. Indeed, if a Sanitary Authority decides to continue or adopt a midden system of excrement disposal, it may be reasonably required of it, consistently with the experience of Hull and other towns, so to carry it out as to avoid the offensiveness and unwholesomeness which have to the present characterised that system. If we insist upon this question of a midden system, it is because we feel assured that in many small towns and rural districts the prevention of excrement nuisance will be more surely and rapidly obtained by the modification of an existing mode of excrement disposal than by the introduction of a new mode.

The common condition for the successful carrying out of the different systems of excrement disposal, among the poorer classes of a population, is, according to the reporters, that *the management of the system should be wholly undertaken and efficiently done*

by the servants of the Sanitary Authority. Such an undertaking, it would appear, is by no means so formidable as it might seem to be at the first glance. In practice it proves to be an economy of means, as well as the method most conducive to an economy of health and life.

The particular conditions which would justify a midden system may be stated as follows:—The *midden* should be detached from the house; it should be constructed of non-porous materials, so as to prevent soakage from it into the soil; it should be roofed, “for all water in it helps decomposition, besides increasing the difficulty of removing its contents;” it should *not* be drained, “for if urine with the liquid and molecular parts of *fæces* be conveyed out of the midden along sewers, there can be no sufficient reason, either commercial or sanitary, for not sending all excrement together along sewers;” it should be of the smallest practicable size, “since largeness of middens means infrequency of emptying, the pits can scarcely be too small;” it should be so arranged, if used as a receptacle for ashes and dry house refuse, as well as excrement, that the former, when thrown in, will cover the excrement.” The *closet* should have its floor and the lower part of the walls constructed of impervious materials, the floor sloping towards the door for facility of cleansing.

“Holding in view, then,” say the reporters, “the conditions which we have seen in actual practice, and combining them so as to give what we think the least objectionable form of midden closet, we would show a closet removed from the house, roofed and floored with sloping flags, the upright of the seat, and the seat itself, moveable; the space under the seat constituting the midden made of non-porous material, with its floor at the ground level sloping backwards. We would see no drain from the midden, as we hold that if the quantity of ashes applied with this construction do not suffice to keep the contents solid, there cannot be any presumption in favour of a midden-closet at all. The roof of the closet serves to keep rain out of the midden, and as much ventilation as procurable is given. The nearest type of this simple, inexpensive, and least harmful midden we find at Hull. The upright of the closet, however, in the Hull plan has the defect of being made of porous material.

“Now, having arrived at this particular modification of the midden-closet as being that from which the worst defects of the system would be found absent, we must direct attention to the one great condition under which alone such a closet is permissible. It is absolutely essential that the removal of its contents shall be very frequently performed; and, as we have said, we consider this one of the chief reasons for giving the preference to the smallest possible midden, for we think it would be waste of time to dwell on a sanitary truth so certain as that

excrement, if it is to be stored at all, along with no better guard than ashes, should be stored for the shortest possible space of time."

The *midden system* may be rendered largely harmless if the midden be constructed so as to contain only a small amount of excrement, and if its contents be removed frequently; the *pail system*, from the facility the pail gives for frequent removal, and for regulating the amount of accumulation, offers numerous advantages over the midden-closet; the *water-system* permits that frequency of removal and freedom from accumulation of excrement which best meets the requirements of public health. Frequency of removal and limitation of accumulation are not essential, although desirable, features in the successful application of the *dry earth system*. Each of those systems, in view of the existing health requirements of English communities, has its peculiar advantages, and none of them can be properly excluded from consideration in dealing with the widely varying conditions under which different populations live.

THE CHOLERA.—RUSSIAN OFFICIAL ACCOUNT OF THE ORIGIN OF THE PRESENT EPIDEMIC IN EUROPE.

IN the *Practitioner* for August we gave a brief account of the early history of the existing diffusion of cholera in Europe. According to the opinion of the Director of the Medical Department of the Russian Government, this diffusion had originated in Russia, but the data upon which this opinion had been formed were only imperfectly known in this country, and Mr. Netten Radcliffe, in an official report on the subject, had shown reason for suspending judgment on the subject until fuller information had been obtained. This information has now been furnished to the English Government by Dr. Pelikan, in an official reply to Mr. Radcliffe's report. The substance of this reply was communicated to the Public Health section of the British Medical Association at its late meeting, and we are now enabled, through the courtesy of the Medical Officer of the Local Government

Board, to lay the more important portions of Dr. Pelikan's rejoinder before our readers.

The cholera epidemic in 1869-71, according to Dr. Pelikan, originated from the small remnants of the preceding epidemic of 1865-66, which penetrated into Russia from the south-west. In the winter of 1866-67 the cholera epidemic prevailed in a mild form in the western zone of Russia. On the approach, however, of warm weather it increased in violence, and in some provinces it reached somewhat considerable dimensions. In 1867 the epidemic was principally concentrated in the south-west provinces of Russia. The epidemic lasted till 1868.

In 1868 cholera existed in the form of a local epidemic of limited extent only in the province of Kief, namely, only in the two villages of Velikaya-Rostofka and Sologubooka, in the district of Liporets, when from the end of July to the middle of August there were only seventy cases of cholera, of which twenty-nine proved fatal. In the other districts of the province of Kief, only isolated cases of cholera occurred. According to official data, the total number of cases in the province of Kief was eighty-three, and that of deaths thirty-five.

Isolated cases of cholera occurred principally during the hot months of summer, in many of the provinces of Russia, but they were considered by medical men to be only cases of sporadic cholera (*Cholera nostras*). The greater number of such cases were observed at St. Petersburg (cases eighty-eight, deaths forty-two), and in Moscow (cases forty-seven, deaths fourteen). In general, however, cases of sporadic cholera (*Cholera nostras*) occurred more frequently in the western zone of Russia, where the number of cases was 400, and that of deaths 130; the corresponding numbers in the central and eastern zones of the Empire being only 150 and 28.

In 1869, cases of cholera began to appear at Kief from the spring, namely, from the 28th May, when a private of the 46th Dnieper Regiment was attacked by the disease in camp. On the 29th June a regimental servant belonging to the Kief Artillery was seized with the disease; and on the 25th July a woman of Kief, called Ardolia Vitughina, was similarly attacked. Between the 31st July and the 13th August, two gunsmiths, who had arrived at Kief from the Ijef works, in the

province of Viatka, and two women belonging to the family of another gunsmith, likewise from the province of Viatka, were attacked by cholera; and on the 20th August a woman from Briansk, who had come to Kief on a pilgrimage, took the disease. All these cases terminated fatally on the same day, or on the next day, with all the symptoms of cholera. From the 24th August cases began to occur daily at Kief; but as they were confined exclusively to persons who lived under sanitary conditions so unfavourable as to be sufficient of themselves to produce sporadic cholera, the epidemical character of the disease was not defined until after it had attacked consecutively several persons who enjoyed the benefit of living under good sanitary conditions. It must, however, be observed that the cholera epidemic was extremely inconsiderable at Kief, notwithstanding the immense concourse of pilgrims from the various provinces of Russia, and notwithstanding also that the pilgrims were subjected to the worst possible sanitary conditions. The total number of persons who were attacked by cholera at Kief in 1869 was 263, while the number of deaths was 152. Such a feeble diffusion of the epidemic, under circumstances so favourable to its retention, caused many Russian medical men to consider the prevalent disease to be not Asiatic cholera (*Cholera Asiatica Epidemica*), but European cholera (*Cholera nostras*). But the onward movement of the disease and its consecutive appearance in the town of Oral (where from the 15th November to the 27th January, 1870, the number of cholera cases was 407, and that of deaths 224), and later in the winter of 1870 at Moscow, very clearly showed its character, and proved that the medical men of Kief had been right when they pronounced the disease to be Asiatic cholera. In other localities the cholera occurred either in small groups of cases and of mortality, or in entirely isolated cases, so that, inclusive of the epidemics in the provinces of Kief and Oral, the total number of cases of cholera in Russia in the year 1869 was barely 911, and that of deaths 510. The relative position of the localities attacked by cholera in 1869 to that of the localities which were seized in 1867 and 1868, and the further diffusion of the disease in 1870, very clearly showed that its progress was from west to east.

The following is a statement of the prevalence of, and

mortality from, cholera in Russia during the five years, 1867-71:—

		Cases.	Deaths.
1867	33,382	13,609
1868	83	35
1869	911	510
1870	20,140	9,446
1871	305,929	116,981

Of the relation of cholera in Kief in 1869 to cholera prevailing contemporaneously in Persia, Dr. Pelikan simply observes, that the infected localities being separated from each other by an immense space entirely free from cholera, the idea of any genetic connection between the epidemics which visited these localities falls of itself to the ground.

We content ourselves for the present with simply reproducing the data upon which Dr. Pelikan's opinion as to the origin of the present diffusion of cholera in Europe has been founded.

ON THE RELATIVE POWER OF CERTAIN ANTISEPTIC AGENTS.

IN the recently published Annual Report of the Army Medical Department (vol. xiii.), Surgeon-Major O'Neal gives the results of an experimental inquiry into the relative power of some reputed antiseptic agents. The experiments were made upon a standard test-fluid prepared by infusing lean^a beef in warm distilled water, the infusion being carefully filtered to get rid of the fat, and used in a perfectly fresh state. This test-fluid was easily managed; and it could readily be obtained at all times of the same strength of organic matter. This strength was determined by measuring accurately a quantity of the filtered infusion from a burette into a platinum vessel, evaporating the fluid, weighing the residue, incinerating this, and again weighing. The infusion was always made of sufficient strength to bear dilution, and the amount of organic matter being ascertained

in the manner above stated, a single calculation gave the quantity of distilled water necessary to be added to each separate infusion, in order to dilute all to one uniform standard of 0.5 grammes of organic matter to 100 c.c. of water.

A quantity of the agent to be experimented on was next weighed, dissolved in an accurately-measured quantity of distilled water, and passed from a burette, in the necessary quantities, into a series of clean, wide-mouthed glass jars. To each of these were added a definite quantity of the beef infusion. Where the agent was not sufficiently soluble in water, the quantities required were separately weighed, made into a paste with a small quantity of distilled water, and then thoroughly mixed with the beef infusion. One of the jars contained beef infusion alone as a standard of comparison.

The whole were then placed in a press in the laboratory, freely exposed to air, and to a moderate light; but not at all to the direct action of the sun. All the antiseptics experimented upon were thus subjected to exactly the same conditions, with the exception of temperature. A self-registering thermometer was, however, left in the press, and the results of its readings recorded. The contents of each glass were examined daily by a microscope with a power of one-eighth, the observations noted, and the results condensed in Table A.

A preliminary experiment was made with the following agents, namely:—1, thymol; 2, zylol; 3, chloride of calcium; 4, sodium chlorate; 5, potassium chlorate; 6, sulphate of zinc; 7, magnesium chloride; 8, calcium bi-sulphite; 9, chloralum; 10, chloride of aluminium; 11, chloride of zinc; 12, permanganate of potassium; 13, sulphate of copper; 15, bi-sulphite of sodium; 16, carbolic acid; 17, bi-chromate of potassium.

The result of this experiment was to show that the first eight had little or no power, and they were therefore dismissed from further consideration, after the correctness of this conclusion had been confirmed by repetition of the experiment. The two first agents, thymol and zylol, were tried because they were at the time used in Dublin, in the treatment of the small-pox. They had the reputation obtained from Germany, of being powerful remedial agents in this disease through exercising an antiseptic influence on the blood. Their

use was, however, shortly discontinued, as they were found to be of no practical value.

During the course of the observations it became manifest that the progress of decomposition might, for the consideration of comparative practical value of the several agents, be conveniently divided into four stages, namely :—

1. From the commencement of the experiment, to the observance of the first animalculæ in independent motion.

2. From this point to the existence of bacteria, &c., in abundance.

3. From the latter point to the first perception of a putrescent smell.

4. To the point when the specimen became so offensive as to require to be thrown away.

It is not pretended that these stages are distinctly marked ; on the contrary, the decomposing fluid passes gradually, and more or less slowly, from one to another. They are, however, held to be useful in considering the value of antiseptic agents in the different objects for which they are used—medical, surgical, and sanitary.

In medicine these agents are administered internally, with the object of exercising, after absorption into the circulation, an antiseptic influence on the blood, as in the case of thymol and zylol for small-pox, and sulphites by Professor Polli in various diseases. But these experiments appear to show that even in the proportions of equal parts of the agent to organic matter, they do not altogether prevent the development of animalculæ, and cause one to doubt the possibility of introducing into the system an amount of any antiseptic sufficiently large to bear to the organic matter of the blood, such a proportion as would exercise any material influence on disease germs contained in it.

A different conclusion may be drawn when the agent is used externally. In the case of carbolic acid, for example, animalculæ were not abundant before the ninth day in the experiment in which it was used in the proportion of one to twelve. When employed as a lotion to an ulcer or wound, the decomposable matter against which it would have to contend would be the organic matter contained in the secretion on the surface of the

ulcer, and however weak the lotion might be, it is probable that the amount of carbolic acid actually applied would be much greater than that of the organic matter. These experiments, therefore, whilst seeming to show that carbolic acid is not a very useful agent for sanitary purposes, do not at all affect its character as a direct local application in surgical practice.

Intermediate between these is the case in which an agent is administered internally, with the object of producing a direct local action. Sulphate of copper, when used in the proportion of one to twelve, retards decomposition so that animalculæ do not become abundant before the fifth day, and smell is not perceived before the twenty-first day. It is suggested that the beneficial influence of this agent in the diarrhoea of typhoid fever is not confined merely to its action as an astringent; but is due also to its antiseptic qualities. Acting as a direct preventative of decomposition in the contents of the bowel, it removes or lessens the amount of septic matter liable to be absorbed by the lower portion of the canal.

The action of the agents experimented upon are, however, mainly considered from a sanitary point of view.

It appeared practically impossible to employ any of the agents for the treatment of sewage in such proportions as would altogether prevent the development of animalculæ, and it became necessary to consider their relative value when used in proportions in which they would prevent the development of animalculæ in abundance for a given period, say ten days; and again in which they would prevent putrescent smell for the same period.

In all the experiments, animalculæ having distinct (however feeble) motion were observed to be present on the second day. This was distinctly ascertained, by repeated trials, to be the case, even with the most powerful of the agents, bi-chromate of potassium. In the light of these experiments—

Chloralum appears to be an agent of little power, as when employed even in so large a proportion as one part to two of organic matter, animalculæ became abundant on the fifth day, and putrescent smell commenced on the seventh. Its price is low, but this does not appear to compensate for its want of power.

Chloride of Aluminium.—In the experiment with this agent, animalculæ became abundant, before the tenth day, in those containing six parts of organic matter, and putrescent smell in those containing eight parts. In the proportions of one, two, and four, fungi were developed in great abundance, and there was a musty but not offensive odour. The agent is expensive, and of no practical value.

Chloride of Zinc.—With this agent animalculæ became abundant on the sixth day, in the proportion of one to two of organic matter, and putrescent smell was observed on the thirteenth day, in the proportion of one to four. In lower proportions fungi were abundant. In addition to its want of power, this agent is costly.

Pernanganate of Potassium appears to have considerable power as a deodorant, but hardly any as a germicide. The experiment lasted a month, and, though no smell was observed in those from fourteen parts of organic matter downwards, they literally teemed with bacteria, vibriones, &c., after the eighth day, even in the experiments containing equal parts of the agent and organic matter.

Sulphate of Copper was under observation for six weeks. Animalculæ became abundant on the sixteenth day in the experiment containing four, and smell was noted on the twenty-first day in that containing twelve parts of organic matter. Those containing a lesser amount remained free from smell throughout, but fungi were developed abundantly in all, except that containing equal parts, which remained free from smell and fungi, showing only a very few animalculæ in feeble motion.

Bi-sulphite of Sodium is of no value. In the experiment containing four parts of organic matter, animalculæ were abundant on the fourth day, and putrescent smell was observed on the tenth. In an equally short time fungi became abundant in the lower proportions, and the higher ones ran rapidly into putrescence.

Carbolic Acid (Calvert's No. 2).—In the experiment containing eight parts of organic matter, animalculæ became abundant on the tenth day, and in that containing six parts not till the twenty-third. Putrescent smell was observed on the twelfth

day in that containing twelve parts. There was a very slight development of fungus in the one containing six parts, but in none of the others. Those containing four parts and less of organic matter showed, throughout the experiments, animalculæ in distinct and tolerably active motion, but they never became abundant. No fungi were formed, and no putrescent smell was developed.

Bi-chromate of Potassium.—In the first series of experiments this agent was tried up to thirty parts of organic matter, and was under observation for thirty-nine days. The agent having resisted up to this point, a second series, in higher proportions, was commenced, and was still going on when this paper was written on the twenty-fourth day. As with all the other agents, animalculæ having distinct independent motion were observed on the second day. In the first series these were few and their motion feeble, requiring occasionally several examinations before being satisfactorily made out; but in the higher proportions, in the second series, they became gradually more numerous, and, in the proportion of one to one hundred and fifty, reached the point of abundance on the eleventh day, and developed slight smell on the twenty-first day. In the experiments containing six parts and less of organic matter there was no fungus. From this proportion upwards it gradually increased, and became abundant at an early date in the higher ones.

This was the most powerful of all the agents tried. It appeared to be quite equal to carbolic acid in preventing the development of fungi, and it was much superior to it and all the others in limiting (none of them prevent altogether) the development of animalculæ. It was also by far the most powerful as a deodorant.

DR. PARKES ON HYGIENE.

I.—MANUAL OF PRACTICAL HYGIENE.¹

WE have so recently had occasion to direct attention to Dr. Parkes' unrivalled "*Manual of Practical Hygiene*" (*Practitioner*, July 1873), that the publication of a fourth edition requires only such brief notice from us as may suffice to indicate the more important modifications which have been made in it. First, the work has been re-arranged so as to adapt it for Civil Medical Officers of Health, as well as for Army Medical Officers. In its present form the Medical Officer of Health will find the work all that he can desire as a text-book. Again, many chapters have been re-written, and all have been carefully revised, so that the work is brought up fully to the present state of knowledge. We need not say more to commend the "*Manual*" to our readers.

II.—REPORT ON HYGIENE FOR THE YEAR 1872.²

Dr. Parkes' Report on Hygiene for the year 1872, as customary, so abounds with matters of interest, that it is difficult for the reviewer to deal with it. After a summary on the more important contributions which have been made to hygienic literature at home and abroad since his previous reports, Dr. Parkes devotes his attention to the following subjects, in the order given:—

1. *Spread of the Specific Diseases*: Paroxysmal features; enteric fever; cholera; dengue; influenza of horses. 2. *Parasitic Origin of Disease*: Entozoic diseases; fungoid diseases; bacteroid diseases. Under the head of paroxysmal fevers, instructive illustrations are given of production of marsh fever by the obstruction of the outlets of subsoil water. Under the head of

¹ "A Manual of Practical Hygiene, intended especially for Medical Officers of the Army, and for Civil Medical Officers of Health." By EDMUND A. PARKES, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School, &c. 4th edition. Churchill. 8vo. pp. 672.

² Thirteenth Report of the Army Medical Department.

enteric fever illustrations of the dissemination of this disease by milk and by water, as recorded by Dr. Ballard and others, are discussed. The sub-section on cholera is chiefly devoted to an elaborate examination of the latest discussions in Germany, particularly in reference to Pettenkofer's views on the subject. Remarkable examples are also given of the introduction of cholera by food, and of its dissemination by drinking water. The first section of the report, that relating to specific diseases, terminates with a brief account of dengue, as recently observed in India, and a briefer account of the late prevalence of influenza among horses in North America. The second section of the report, that relating to the parasitic origin of diseases, treats successively of the diseases produced in man by entozoa, the fungoid origin of disease, and the bacteroid origin of disease; the latter question first considered in its general aspect, and next successively with regard to the questions (a) of the specific diseases supposed to be caused by bacteria, and (b) of the non-specific diseases attributed to bacteria.

The principal interest of the report centres in Dr. Parkes' discussion of Pettenkofer's views as to the influence of the ground and of fluctuations of the ground-water in the development of cholera, and as to the bacteroid origin of infectious diseases.

We shall not attempt to follow Dr. Parkes' argument on the first of these subjects, and his examination of the views of recent German writers on the same subject. We are chiefly concerned with the conclusion which Dr. Parkes has himself arrived at on the question. This is stated in the following words:—

"To me it certainly appears, at the present moment, that Pettenkofer's ground and ground-water theory is less probable than it appeared to be some years back. That the ground has an influence is highly probable, and is in fact a very old opinion; but when it is seen how very indefinite even Pettenkofer's own evidence is, and how uncertain are some of the cases he relies upon, I think it must be admitted that the enigmatical spread of cholera has not been cleared up by him.

"If without presumption I may venture to criticise Pettenkofer's mode of looking at this subject, I should say he has allowed himself to be too much impressed by the instances in which cholera does not spread after introduction. In attempting to explain, instead of simply admitting that we cannot yet explain such cases, he has passed beyond the boundary of ascertained facts, and in so doing appears to me to have lost his hold on what are really truths. The view

which looks on the cholera-poison as being carried by men, and then increasing and spreading more or less in various ways, by water, air, or food, according as it meets with favouring conditions, is more in accordance with facts than the view which assumes that two unknown quantities must be brought together in order to evolve a third.

"One point may be noticed in this discussion, the unhesitating belief of all parties (Küchenmeister, Sander, and Pettenkofer) that cholera is spread only by human intercourse. With them this point has passed out of the circle of controversy: they consider it settled. The views of Bryden, interesting as they are to them, have apparently made no impression; and the wind-driven theory of cholera spread has found no acceptance at their hands."

In regard to the bacteroid origin of infectious diseases, Dr. Parkes holds, from a revisal of the observations which had been made during the year he had under examination, that we "must yet hesitate before considering" the parasitic theory of infectious diseases has been established. The following is the substance of Dr. Parkes' observations on the specific diseases supposed to be caused by bacteria:—

"As regards cholera, the careful observations of Drs. Lewis and Cunningham, in Calcutta, seem to have disproved the possibility of either fungi or bacteria being the cause of cholera.

"Very different is the case of some other diseases, if the observations of several observers, and especially of Professors Coze and Feltz, of Strasburg, may be trusted. They assert that in the blood of septicæmia, typhoid fevers, and puerperal fever, there is present a linked or chained bacterium, which they term (after Dujardin) *Bacterium catenula*, and they believe that the growth of these bacteria is the efficient cause of these diseases; indeed they go further, and assert 'that every infectious substance is of bacteriferous nature,' although the form of the bacteria is different in small-pox, scarlet fever, and measles from what it is in typhoid. The activity and specificity (to coin a word) of bacteria arise, they conceive, not because they are the carriers of a special soluble principle, but from the rapidity of their multiplication; the form and size of the bacteria depend on the soil in which they are reproduced and multiplied. The various phenomena of the infectious diseases are supposed to be dependent on pathological localisation, and these localisations arise from the rapid growth of bacteria; from the detritus formed from them when dead; from the leucocytes which are often simultaneously developed in excess, and from fibrillary deposits of fibrine.

"The authors attempt to prove these assertions (which, if correct, would largely modify our views of infectious diseases) by microscopic examination, by experiments, and by clinical observations. The diseases treated of are pyæmia and septicæmia, typhoid fever, small-pox, scarlet fever, measles, and puerperal fever. They distinguish between septicæmia from pyæmia, as is now usually done, though the two are often mixed.

"With respect to septicæmia, the authors showed, in 1866, that the blood of animals infected with a poisonous liquid is itself infectious, and that the red globules of the blood are then altered, and a multitude of bacteria, &c., exist, and that in successive inoculations death ensues more and more rapidly, so that the infectious element seems to gain in activity as it passes through the same organ-

isms. They now support these operations by numerous experiments on rabbits, which are, on the whole, strongly confirmatory of those made by Burdon-Sanderson, and presently to be recorded. Passing over the changes they describe in the red globules, in the white cells, and in the fibrine (fibrillary deposits), and referring only to the infusoria, they found always single, double, or multiple corpuscles, usually in chains, but looking like little worms. Sometimes they saw the whole element with its greyish or slightly yellowish colour; sometimes merely a pale or blackish point (according to the illumination), which was owing to an element seen from above, and presenting one end; these points measured .0016 mms., the complete elements .004 to .02 mms. in length and .066 mms. in thickness. They had little activity, and resembled bacteria rather than vibriones. They die in the body, and especially in the lungs. The cause of death in septicæmia is considered to be a profound alteration of the blood, tending to, but not usually reaching putridity, and induced by the countless myriads of bacteridia.

"In pyæmia there is also often the presence of bacteria, but because there is usually coincident septicæmia, there is in addition or alone numerous embolisms arising from substances introduced mechanically into the circulation (pus, white globules, substances from inflamed veins), and these may exist without any bacteria. The authors thus draw a broad line between the two affections.

"In typhoid fever, experiments were made with blood taken from human typhoid patients and injected into the blood of rabbits; an infectious disease (with implication of Peyer's patches) was given to the rabbits, whose blood was able to communicate the disease to other rabbits. The dried and powdered blood of an infected rabbit preserved its power to produce infection for more than a year. In the blood of men and rabbits there were (in addition to alterations in the red and also largely in the white globules) a great number of bacteria, smaller in size than in the septicæmic blood; they were often divided, as in septicæmia, into three, four, or five segments (*B. catenula*).

"The evidence of the presence of bacteria in typhoid blood seems to be complete, but it is, of course, still an open question whether they are the cause of the typhoid fever, and it even appears from some of the post-mortem histories that bacteria were not always found in the blood. The examinations of typhoid blood during life appear also to have been very few in number.

"With regard to small-pox, the observations of Chaveau and Sanderson have proved that the infectious property resides in the solid glistening particles. Whether these glistening particles are bacteroid or not is a matter of dispute. Beale, their discoverer (in 1865), considers that in vaccine lymph they consist 'of a peculiar kind of living matter, the smallest particle of which, when supplied with its proper pabulum, will grow and multiply.' Variolous pus contains similar particles, and in both cases they are portions of bioplastic matter. Dr. F. Cohn has now convinced himself that these particles are 'living uni-cellular organisms belonging to the group of the so-called globular or sphere bacteria' (*Kugel bacterien*). The authority of Ferdinand Cohn in a matter of this kind is so great that it would seem we must admit that the small points described first by Beale are really bacteria; at the same time Beale has worked with such high powers and is so expert in these investigations, that a further inquiry may perhaps still be demanded before Cohn's view is definitely admitted.

"Coze and Feltz state that they found articulated bacteria in immense quantities in the blood of the human subject attacked with small-pox, and on injecting this blood into rabbits, they produced feverishness and bacteria similar to those in the variolous blood which were found in the rabbit's blood. In variolous blood they have also found round globules armed with points, which

they consider to be bacteria fixed to a globule. They also noticed many deposits of fibrine, as in typhoid fever. The kind of bacteria, they state, resembled the *B. bacillus* of Pasteur and the *B. termo* of Müller, and are quite different in aspect from those of septicæmia and enteric fever. As in septicæmia, the transmission of the virus through successive rabbits seemed to heighten the virulence of the poison.

"In scarlet fever blood these authors have also found bacteria which, when transported into the system of rabbits, largely increased in size. In man their length was '0006 mms, and their breadth '0002 mms.; in the rabbit they were eight times as big. The injection of human scarlatinal blood into rabbits produced a fatal feverish disease.

"In measles, extremely small and mobile bacteria were found, but the blood of measles was not toxic to rabbits. The authors state that the blood taken from the part of the skin covered with eruptions contains many more bacteridia than the blood taken from the sound part of the skin, and believe therefore that there is a connection between the eruption and the number of bacteroid elements; in other words, we presume that they mean to affirm a great local development of bacteria in the vessels of the skin.

"In the blood of puerperal fever, the authors found (in all infectious diseases) the following changes; deformed red globules, augmentation of leucocytes, fibrillary deposits of fibrine and chains of bacteria, and the same characters were found in the blood of rabbits poisoned with injection of puerperal fever.

"The observations of Coze and Feltz on septicæmia are strongly supported by the beautiful experiments of Burdon-Sanderson and Klein, and by the observations of Klebs, of Davaine, and several others. Sanderson and Klein have found bacteria in septicæmic and pyæmic blood, and have also confirmed in another way the curious observation of Coze and Feltz, that by successive inoculations the virus increases in intensity. If a pyæmic fluid is transferred to the peritoneum of a guinea-pig, and is allowed to remain there for a couple of days, and is then introduced into another animal, its toxic power has so increased that it has acquired the most deadly activity. 'All such extremely active liquids were crowded with bacteria of a peculiar character, the increased number of which seemed to be in proportion to their toxic properties.'

"Dr. Sanderson believes that bacteria 'afford a characteristic, by which we may distinguish the products of infective inflammations from those which are non-infective, and that their number affords an indication of the degree of infectiveness.' Dr. Sanderson, however, believes, from actual experiment, that the ordinary bacteria of putrefaction have no toxic action, and he is not prepared at present to say that the bacteria of septicæmia are the toxic agents; he regards them as the inhabitants of infective fluids, and as very probable carriers of infection.

"The work of Klebs, which is based on observations in Carlsruhe during the war of 1870-71, is remarkable in various surgical aspects, but in none more than in the statements made as to the chief cause of death after wounds. He notices (p. 105 *et seq.*) that the differences in the amount and fatality of wound-fevers and pyæmia and septicæmia cannot be dependent on the physical condition of the pus, for it is of all kinds of fineness and coarseness. A fine microscopic examination has convinced Klebs that the cause of secondary pus formation lies in the presence of 'putrefaction fungi' (Fäulnisspilzen), which he terms microsporon septicum. He confirms entirely the statements of Rocklinghausen and Waldeyer, and though he does not allude to him, his observations seem confirmatory of those of Lister. The method in which it is sought to prove that this parasite is a pus

and fever-making cause is twofold ; an anatomical and a physiological. With regard to the first, the microscopic examinations showed bacteria, vibriones, and monads in almost all cases of wound-secretions ; the bacteria are often motionless, rod-like bodies, often joined together, so as to form long-jointed fibres ; there were also numerous microspores, that is, extraordinarily small glistening particles, either free, and then having oscillatory movement, or in groups (zooglæa-form), or in chaplets. These bodies are found in good as well as bad pus, but are sometimes wanting in good pus. These parasites are, Klebs thinks, the same as those described by Hueter and Tommassi in the diphtheritis of fresh wounds. Commencing in the secretion on the outer surface, the parasites attach themselves to the soft parts, and colonise there, and form zooglæa-masses, just as they may be artificially grown on the mesentery of the frog. The colonies extend themselves on all sides, unless they meet with a cleft, which interposes a chasm or produces mechanical compression, which destroys them. They destroy the surface in this way, and then they penetrate into the lymph and blood-vessels, and get to the inner organs. Sometimes they eat through the wall of a vessel from outer to inner coat, and, getting into the vessel, cause coagulation. Of the parasite, many penetrate into soft tissues, which they easily destroy, though the hard bones and tendons resist them. They penetrate into the interstices of the loose connective tissues either directly, or are assisted by those forces which aid in moving the lymph. This latter mode is very important. When the parasite passes into the connecting tissue interspaces the permanent cells are destroyed by the mechanical pressure, but in the spaces are found wandered white cells ; the parasites pass into these wandering cells (which have come from the wound surfaces apparently, and contain often hæmatoidin), and then are traced into the lymphatic glands. A general infection of the system occurs very slowly in this manner, and the spread of the parasite-holding cells beyond the lymphatic glands is very difficult to prove. The parasite arrives in the muscles from the connective tissue spaces ; then during the contraction of the muscular fibres, the spaces between them are widened, and neighbouring fluids are drawn by aspiration into the spaces ; in the succeeding period of muscular relaxation the solid particles in the fluid are not driven out again ; then occurs the well-known interstitial myositis and pus building.

“The general infection of the body arises most commonly by the infection of the blood, and the transference to various parts ; the little thromboses which are found behind the valves in the veins are caused by the adherence of the microspores to the walls, by their colonisation and growth there, and by the irritation and pouring out of a fibrino-plastic substance on the walls, a coagulating influence is exerted on the blood, and perhaps also the anti-coagulating influence of the walls pointed out by Brücke is removed. These thromboses may remain and often form pus. Then the organs suffer, the lungs especially, from the mechanical arrest of numerous solid parts, or sometimes from coagula in the vessels.

“Klebs enters at considerable length into the thrombosis and coagulation in the lung vessels in septicæmia, and then passes to the hepatic abscesses, which seldom arise from emboli, but from the distribution of the microspores in the capillary vessels, which are distended, press on the liver cells, and destroy them. Klebs concludes that the opinion which looks upon these parasitic elements as merely unessential and accidental attendants of suppuration and inflammation must be given up, as complete proof has been obtained that the local mycosis precedes these processes. But in addition, Drs. Zahn and Tiegel have succeeded in filtering the parasitic masses. The clear fluid caused heavy but transient fever, but never caused local suppurations ; the same fluids containing the parasites

caused extraordinarily widespread suppurations. Zahn's experiments have, however, been doubted.

"Finally, in the splenic apoplexy, or carbuncular diseases (Milz-brand) of sheep and cattle, in which bacteria were discovered by Branell, Davaine, and others, it has been asserted by Dr. V. Grimm, of St. Petersburg, that no bacteria were found in the blood during life. But this has been contradicted in a note by Dr. Semmer, of Dorpat, who not only from his own observations, but from those of Unterberger, in Dorpat, and Nayorski, in St. Petersburg, entirely confirms Branell's statement of the constancy of the occurrence of these bacteridia in the blood of carbuncular disease."

It is much to be regretted that Dr. Parkes' important annual reports on the progress of hygiene are not published in a separate form.

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THE ACTION OF THE CINCHONA ALKALOIDS AND SOME OF THEIR CONGENERS ON BACTERIA AND COLOURLESS BLOOD-CORPUSCLES.

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CINCHONA-BARK and quinia are universally allowed to be anti-periodic, antiseptic, and antiphlogistic. The first of these adjectives denotes their specific influence over the fevers and other morbid states caused by the paludal miasm; the second, their curative virtue in septic conditions of the blood; the third, their power to restrain the migration of the colourless blood-corpuscles from the vessels, and thereby to arrest, or at any rate to control, the violence of suppurative inflammation.

These properties rest on evidence gathered at the bed-side. No sooner, however, do we quit the ground of clinical observation than our footing becomes insecure. What is the *nexus* between these therapeutic virtues? Do they depend on the same or on different properties of the alkaloid? There are two ways of arriving at a solution of this problem. One, by inquiring into the pathology of the diseases over which bark exerts a salutary power, and tracing some common factor in all of them which the drug may be supposed to influence. The

other, by studying the action of the drug analytically, by investigating its effect on the simple processes of which the complex whole we term "disease" consists.

The present is a contribution to the latter branch of the inquiry. The antiseptic action of the four chief alkaloids of the cinchona-bark is studied experimentally, and compared with their controlling power over the migratory movements of the colourless blood-corpuscles. The area of comparison is extended to include several other substances whose precise relation to the cinchona alkaloids is still under discussion. The results bear primarily on the antiseptic, and what I have termed the antiphlogistic properties of quinia and its congeners. Indirectly, they touch on the question of their antiperiodic virtues.

The question of the antiseptic efficacy of quinia has grown much in importance of late years. Septicæmia, some of the specific fevers, the doctrines concerning the material vehicle of contagion, the part played by bacteroid organisms in relapsing fever, are all connected with the question of antiseptic medication! Attempts have also been made to ascribe intermittent fever to the agency of zymotic organisms, and to account for the antiperiodic power of quinia by its action upon them. Unfortunately, no such organisms have yet been shown to exist in the blood of patients suffering from the effects of marsh miasmata; and even the *a priori* probability of the theory has to contend against a host of adverse clinical facts. However the question may ultimately be decided, it is interesting to ascertain whether there be any parity between the antiseptic and the antiperiodic properties of drugs.

In judging of experimental results, it is not necessary to assume that the action of the drugs in the blood is *identical* with their action in the laboratory. From a similarity of physiological action, we may justly anticipate some analogy of therapeutic effect; from their quantitative equivalency outside the body, we may infer a like equivalency when they are introduced into the current of the circulation; but repeated failures have shown that we may go no further,—that we cannot be certain how far their mode of operation is the same in the two cases.

I. THE ANTISEPTIC ACTION OF THE CINCHONA ALKALOIDS AND SOME OF THEIR CONGENERS.

THE singular freedom from decay enjoyed by all the varieties of cinchona bark must have been noticed by pharmacutists from a very early period. The permanence of decoctions and infusions of bark, as compared *e.g.* with those of quassia, forces itself on the attention of the most careless dispenser. It was not till the middle of the last century, however, that the antiseptic qualities of bark were experimentally investigated. In the *Philosophical Transactions* for 1750, there is a paper by Sir John Pringle, entitled "Some Experiments on Substances resisting Putrefaction," giving a series of observations on the comparative antiseptic power of a variety of substances, including "the Bark." The author exposed bits of meat, steeped in solutions or decoctions of the antiseptics employed, to a temperature of 94°—104° F. for several days, and noted the kind and degree of putrefactive change which they underwent. His observations are reduced to a common standard, viz. the antiseptic power of a solution of common salt of known strength.

These experiments laboured under several disadvantages; the true nature of putrefactive decay was still unknown, more especially as regards the part played by bacteria; the substances employed were not chemically pure; and the sense of smell, as a test of decomposition, is neither wholly trustworthy, nor capable of quantitative expression. Notwithstanding these drawbacks, however, the results obtained were, upon the whole, singularly correct, and have been corroborated by the more exact researches of later enquirers. With regard to cinchona bark, Pringle makes the following statement: "The common neutral salts, however powerful in resisting putrefaction, are inferior to some resinous substances, and even some vegetables, which I have tried. Thus myrrh, in a watery menstruum, was found at least twelve times more antiseptic than sea-salt." After alluding to camphor, Virginian snake-root, and chamomile flowers, he goes on to say: "the Jesuit's bark has the same extraordinary quality" (*i.e.* that of hindering putrefaction); "and if I have not found it so strong as the two substances last mentioned, I

impute that in part to my not being able to extract its embalm-ing parts in plain water." The experiment is described thus: "I made a strong decoction of the bark, and infused a piece of flesh in two ounces of it strained; which flesh never corrupted, though it remained two or three days in the furnace after the standard" (flesh steeped in brine) "was putrid. In this time, the decoction became gradually limpid, whilst the grosser parts subsided; by which it appears that a most minute portion of the bark, intimately mixed with water (perhaps less than of the snake-root or chamomile flowers) is possessed of a very extraordinary antiseptic Force."

His theory, as may be expected, falls short of his observations; he is inclined to attribute the antiseptic virtue of the bark to its astringent properties. "By the success of the bark in so many putrid cases, it should appear that astriction had no small share in the cure, and indeed the very nature of putrefaction consists in a separation or disunion of the parts."

Pringle's observations lay forgotten for upwards of a century. Even Briquet, in his elaborate and valuable monograph,¹ refers very briefly to Pringle's results, and obviously confounds the antiseptic with the astringent properties of the bark:—"Ces tentatives montrent seulement l'influence de l'action chimique du tannin sur la putréfaction, mais elles ne prouvent rien sur la prétendue propriété antigangréneuse attribuée au quinquina" (p. 40). It was not till the researches of Professor Binz and his pupils,² that the high place of quinia among antiseptics became generally recognized. And this recognition has already thrown much light on the efficacy of quinia in cases where its use had previously been ascertained by clinical trials, and is likely to extend the sphere of its future application.

No really adequate conclusions as to the antiseptic power of the Cinchona alkaloids were possible so long as the intimate nature of the putrefactive process remained unknown. The circumstance that it depends on the evolution of a multitude of extremely simple organisms, narrows the area of the inquiry very materially. The question as to the antiseptic properties of

¹ "Traité Thérapeutique du Quinquina," Paris, 1855.

² "Experimentelle Untersuchungen über das Wesen der Chininwirkung," Berlin, 1868.

quinia and other substances resolves itself into the question of their action upon microzymes.¹

Before proceeding to speak of the effect of drugs on the vitality of microzymes, it is essential to define the modes in which their vitality may manifest itself. The power of spontaneous locomotion, and the power of undergoing unlimited multiplication in suitable media, are the criteria on which we have to rely in deciding whether they be alive or dead.

Take first their power of spontaneous movement. To study this in a drop of water swarming with microzymes, it is necessary, or at least desirable, to employ some method by which the water may be kept from evaporating too rapidly, while a continuous supply of oxygen is allowed to have access to it. Unless these precautions be taken, the locomotive activity of the microzymes speedily ceases. The drop of liquid may be inverted on a thin cover-glass over the well of a Stricker's stage; if the drop be of proper size it hangs without much vibration. The air in the well is renewed through tubes communicating with the exterior, and serving for the introduction of gases when the apparatus is used as a gas-chamber. Evaporation is guarded against by the previous introduction of a drop of distilled water into the well. In this way, the same drop may be kept under observation for hours with one of Hartnack's or Gundlach's immersion-lenses.

On examining such a hanging drop, we notice that a number

¹ This term, employed by Dr. Burdon Sanderson in his "Report on the Intimate Pathology of Contagion" (*Quarterly Journal of Microscopic Science*, Oct. 1871), is more adapted to my purpose than the specialised nomenclature promulgated by Cohn ("Beiträge zur Biologie der Pflanzen," 1872). In the following pages it will be held to include the following among the *Schizomycetes*: *Bacterium termo*, *B. lineola*, *Bacillus subtilis*. The genera *Spirillum* and *Spirochaete* never appeared in my test-liquids; even the genus *Vibrio*, in the narrow denotation given to it by Cohn, was not met with. The *Sphaerobacteria*, whose precise morphological limits even Cohn leaves somewhat uncertain, I put aside altogether; they are destitute of the power of spontaneous movement, which is a distinguishing characteristic of all those forms which I include under the head of microzymes. None of the chromatogenous or pigmentary *Sphaerobacteria* ever showed themselves in my solutions, except when purposely introduced; and the non-chromatogenous species are not easily to be distinguished from the *Zooglæa* formed by a variety of the rod-like bacteria. Indeed, the minute spherical particles which were invariably present in conjunction with *Bacterium* and *Bacillus* might or might not have been early stages in the development of the latter forms. As a test of putrefactive change, the rod-like organisms, capable of spontaneous locomotion, which I have grouped together under the head of microzymes," are amply sufficient.

of rod-like Bacteria are absolutely motionless upon the under surface of the cover-glass, to which they seem to adhere. At the extreme periphery of the drop they become very numerous, and are closely aggregated, forming a narrow zone of no movement. In the stratum just below this, the microzymes are all in rapid motion. A considerable number, usually the majority, are simply oscillating or vibrating; their movement is wholly passive (Brownian), and is exhibited by minute solid particles of any kind. Others, however, are moving *actively*, either with or without change of position. In the former case, that of translation, we see a rod-like or hourglass-shaped particle dart across the field with lightning-like velocity, or swim about more slowly, avoiding some obstacles with seeming intention, coming into collision with others. In the latter, we have the gyratory movement of a staff-shaped Bacterium, which, keeping one end fixed, makes the other revolve in a circle with extreme rapidity; or the sudden swaying movement of *Bacillus subtilis*, reminding the observer of that of *Oscillatoria*.

All these varieties, and many others, of active movement, afford conclusive evidence of the vitality of the organisms which exhibit them. But the entire absence of movement, or of all except passive movement, is no proof of death; it is a rigidity or torpor which lasts as long as the cause to which it is due continues to operate, and which may at any moment be removed by the removal of its cause. It does not bar the reproductive power of the microzymes.

The fertility of the microzymes contained in any liquid is tested by introducing a drop of the liquid into a tube containing 4 c.c. of a solution primed with the needful pabulum.¹ The tube and pipette should be carefully superheated beforehand, and

¹ The test-solution I first used was Pasteur's fluid, slightly modified by the substitution of minute quantities of sulphate of magnesia and phosphate of potash for the yeast ash, which it is extremely difficult to obtain pure, at least without special appliances for incineration. The formula is as follows :—

Cane sugar, 10 grammes.
Ammonium tartrate, '5 grs.
Magnesic sulphate, '05 grs.
Potassic phosphate, '05 grs.
Distilled water, 100 c.c.

Finding that the sugar favoured the exuberant growth of *Torula* and *Penicillium*, I omitted it, and found that the liquid answered my purpose quite as well

the test-liquid thoroughly boiled, to obviate the risk of accidental contamination; another tube, containing the same test-liquid, but not inoculated, should always be reserved for comparison. The tubes are then stopped with cotton-wool and put aside in a warm place (65° to 75° F.) for two or three days. At the end of that time, the inoculated liquid—if the microzymes retain their vitality—will be found opalescent, or even milky, while that reserved for comparison is still clear. A day or two later, a gelatinous pellicle forms on the surface of the infected liquid, and a white sediment of dead or torpid bacteria begins to accumulate at the bottom of the tube. On examining a drop of the liquid under the microscope, it is found to swarm with locomotive microzymes.

It seemed desirable in the first place to ascertain the precise nature of the action exerted by quinia upon microzymes. Previous inquiries had been too exclusively directed to the presence or absence of putrefaction in the substances experimented on, as a test of the efficacy of the antiseptic agent; the following experiments were primarily designed to throw light on the intimate nature of those actions by which the putrefactive changes are stayed or retarded—on the specific influence of the drug upon the life of the saprogenic organisms.

The destructive effect of heat on microzymes is universally admitted, though the precise temperature at which their vitality is permanently arrested is still under discussion. Indeed the discrepancy between the results arrived at by different investigators is at first sight bewildering. Bastian (*Proc. R. S.* 1873) concludes that a temperature of 70° C. completely destroys all germs, whether in saline solutions or organic infusions. Cohn (*l.c.*) states that exposure to a temperature of 62° C. for a period

without it. After I had come across Cohn's essay, I employed his nutrient solution. It is prepared thus:—

Ammonium tartrate, 1 gramme.
Potassic phosphate,
Magnesic sulphate, of each, 5 gra.
Calcic phosphate, .05 gr.
Distilled water, 100 c.c.

The calcic phosphate is very incompletely dissolved, and the liquid must always be filtered before it is used. The addition of 7.5 grammes of cane-sugar and 1 c.c. of dilute HCl adapts it admirably for the cultivation of fungi such as *Penicillium*, &c. ———

of one hour is fatal to the vitality of Bacteria. Lex¹ found a temperature of 127° C. insufficient to kill them; and Calvert (*ibidem*) requires a temperature of 204° C. for their complete destruction. In the test solutions employed by me, no microzymes were ever developed when the liquid had been boiled for a few minutes in a superheated tube, and guarded from subsequent contamination by keeping the mouth of the tube plugged with cotton-wool. I should add that the solutions were never allowed to contain any visible particles of solid matter, for the presence of such particles seems undoubtedly to raise the lethal temperature by a number of degrees varying in each case.

Notwithstanding these differences of opinion, since heat, as a destructive agent, has been more thoroughly studied than any other antiseptic, it appeared suitable to take its effects as a starting-point and standard of reference for further comparison.

EXPERIMENT 1.²—A drop of Cohn's solution, swarming with microzymes, was placed on a cover-glass over the well of Stricker's stage, in the manner described above. The temperature was gradually raised from 20° to 40° C. The locomotive activity of the microzymes increased as the temperature rose, attaining its maximum at 40°. As the heat was allowed to wax beyond this point, their activity began to wane; and at 60° C. they were all motionless. This temperature was kept up for five minutes; at the end of which time not many were to be seen in the field; the few remaining ones exhibiting only Brownian movements, and seeming to be shrivelled and altered in structure. On searching the drop carefully, not a single locomotive particle could be detected.

The above experiment shows that the locomotive power of microzymes may be arrested by a temperature below that which is assigned by the most moderate observers as the lethal one. That their reproductive power is not in any way impaired by the heat which renders them motionless, is shown by the following experiment:—

EXPERIMENT 2.—A drop of Cohn's solution, teeming with locomotive microzymes, was exposed on Stricker's stage to a

¹ Quoted by Parkes, "Practical Hygiene," 1873, p. 476.

² The experiments detailed in this paper are selected for the purpose of illustration; it must be understood that they were repeated and varied.

temperature of 66°—70° C. during a period of five minutes. The cover-glass had previously been roasted in the flame of a spirit-lamp. At the end of the time, no locomotive particles could be detected in the drop. The cover-glass was then removed with superheated forceps and dropped into a tube containing 4 c.c. of boiled Cohn. The tube itself had been previously roasted, and was now plugged with cotton-wool. For purposes of comparison, a second tube was treated in exactly the same way, save that the cover-glass dropped into it had simply been exposed to the air for five minutes after it had been heated to redness. A third tube was simply charged with the test-solution, and then plugged. After the lapse of forty-eight hours, the liquid in the first tube was opalescent throughout, and was found to swarm with microzymes; while that in the second and third remained perfectly clear.

A temperature of 100° C. (and probably much less) abolishes both the locomotive and the reproductive power of the microzymes; both the phenomena referred to above as the manifest indications of their vitality.

The next experiment was designed to ascertain the proportion of quinia hydrochlorate necessary to arrest their power of spontaneous movement.¹

EXPERIMENT 3.—3 c.c. of Cohn's solution, full of active microzymes, were mixed with 1 c.c. of a 1 per cent. solution of quinia hydrochlorate (1 in 400). A drop of this, examined on Stricker's stage, was seen to contain a vast multitude of quiescent microzymes, among which a few still travelled lazily about. When the proportion of the quinia salt was raised to 1 in 250, no locomotive particles could be detected. The highest power I had at my disposal failed to show any alteration in the appearance of the bacteria, such as that which is brought about by heat.

The next point was to determine whether the motionless state induced by the above dose of quinia was one of real or apparent death.

¹ Throughout the inquiry, the hydrochlorates of the four cinchona alkaloids were employed. Quinia hydrochlorate is perfectly neutral, and more soluble than any other of its commercial salts. The alkaloids were obtained from Messrs. Howard and Son; the other active principles from Messrs. Hopkin and Williams.

EXPERIMENT 4.—3 c.c. of a 1 per cent. solution of quinia hydrochlorate were thoroughly mixed with 1 c.c. of pea-water (prepared by steeping a split pea in 4 c.c. of distilled water for a few days) teeming with microzymes (1 in 133). Three superheated tubes were then charged with 4 c.c. each of boiled Cohn. The first was inoculated with a drop of the above mixture; the second with a drop of pea-water; the third was not inoculated. They were all plugged with cotton-wool, and put under a bell-jar. After three days, the first was faintly opalescent, and contained a multitude of excessively minute microzymes; the second was more cloudy; the third continued perfectly clear.

Thus we see that as soon as microzymes are liberated from the inhibitory influence of the quinia, they resume their vital properties and multiply almost as rapidly as usual. Distinguishing between a temporary and a permanent arrest of function, between a state of torpor and one of death, we are brought to the conclusion that quinia, in such proportions as those employed above, exerts an inhibitory, not a lethal influence on microzymes. Its action is analogous to that of a temperature of 60° C., not to that of a temperature of 100° C. Or it may be compared with the torpor induced by the deprivation of oxygen.

That there exist substances which, in the same minute proportions, exert an immediately fatal influence on microzymes, is shown by the following experiment:—

EXPERIMENT 5.—Four superheated tubes received 4 c.c. each of boiled Cohn. The first was inoculated with a drop of boiled pea-water; the second with a drop of pea-water containing .75 per cent. of mercuric chloride (1 in 133); the third with a drop of pea-water rendered strongly alkaline with liquor sodæ; the fourth with a similar drop acidulated with hydrochloric acid. The tubes were plugged with cotton-wool and put under a glass shade. Three days later, all four tubes remained quite clear, while a corresponding one, inoculated with simple pea-water, was quite cloudy.

That quinia, in larger proportions, may produce similar fatal effects, is highly probable. The sparing solubility of even its hydrochlorate prevented the accurate determination of this point; and it is the less interesting, since the proportion of the alkaloid

employed in the foregoing experiments is larger, or at any rate quite as large, as could safely be introduced into the circulating blood of an animal.

The next step was to determine the proportion of quinia hydrochlorate needful to arrest the multiplication of microzymes in inoculated test-liquids, so as to have some standard with which to compare the antiseptic efficacy of the other alkaloids. And here it was found that Cohn's solution was unsuitable, inasmuch as a variable proportion of the quinia salt crystallized out, and so vitiated the results of the experiment. After repeated trials, a concentrated decoction of beef, free from fat, and twice filtered, was chosen as the most convenient test-liquid. Its reaction was neutral.

EXPERIMENT 6.—Four tubes were charged as follows:—

1. Beef-tea, 1 c.c.; distilled water, 3 c.c.
2. Beef-tea, 1 c.c.; distilled water, 2 c.c.; solution of quinia hydrochlorate (1 per cent.), 1 c.c.
3. Beef-tea, 1 c.c.; distilled water, 1 c.c.; quinia solution, 2 c.c.
4. Beef-tea, 1 c.c.; distilled water, 0; quinia solution, 3 c.c.

Each of the tubes was thus made to contain the same proportion of nitrogenous matter and salts in 4 c.c. of water. The first contained no quinia; the second .25 per cent.; the third .5 per cent.; the fourth .75 per cent. Each tube was then inoculated with a drop of pea-water, and plugged with cotton-wool. Six days later, the liquid in the first tube was quite cloudy, while that in the other three tubes remained clear, and yielded no trace of organisms under the microscope.

Similar experiments, performed with smaller proportions of the quinia salt, showed that its inhibitory influence diminishes directly as the dose; the test-liquid varying from perfect clearness to a milky opacity. For purposes of comparison, a strength of .25 per cent. was found to be most convenient; smaller proportions allowing the test-liquid to become distinctly opalescent during the time the experiment lasted, in degrees inversely proportionate to the amount of the drug present, but incapable of verbal expression.

The following are some of the trials made with neutral solutions of the hydrochlorates of cinchonina, quinidine, and cinchonidine.

EXPERIMENT 7.—Three tubes were charged as follows:—

1. Beef-tea, 1 c.c. ; distilled water, 3 c.c. ; solution of cinchonia hydrochlorate (.5 per cent.), 4 c.c.

2. Beef-tea, 1 c.c. ; distilled water, 0 ; cinchonia solution, 7 c.c.

3. Beef-tea, 1 c.c. ; distilled water, 7 c.c.

Each tube was inoculated with a drop of pea-water and plugged. The first contained .25 per cent., the second .44 per cent. of the drug. In two days' time, the third tube was opalescent ; the first two perfectly clear. After the lapse of five days, the latter also were opalescent, the first more so than the second. The third was by this time quite milky.

EXPERIMENT 8.—Precisely similar to the last, substituting cinchonidine hydrochlorate for cinchonia. The third tube alone became opalescent, the first two remaining perfectly clear a week after.

EXPERIMENT 9.—Six tubes were charged as follows:—

1. Beef-tea, 1 c.c. ; distilled water, 7 c.c.

2. Beef-tea, 1 c.c. ; distilled water, 5 c.c. ; solution of quinia hydrochlorate (1 per cent.), 2 c.c. ; (proportion of drug, .25 per cent.).

3. Beef-tea, 1 c.c. ; distilled water, 3 c.c. ; solution of cinchonia hydrochlorate (.5 per cent.), 4 c.c. ; (proportion .25 per cent.).

4. Beef-tea, 1 c.c. ; distilled water, 3 c.c. ; solution of cinchonidine hydrochlorate (.5 per cent.), 4 c.c. ; (proportion .25 per cent.).

5. Beef-tea, 1 c.c. ; distilled water, 3 c.c. ; solution of quinine hydrochlorate (.5 per cent.), 4 c.c. ; (proportion, .25 per cent.).

6. Beef-tea, 1 c.c. ; distilled water, 0 ; quinidine solution 7 c.c. ; (proportion, .44 per cent.).

The tubes were all inoculated with pea-water and plugged. Three days later, the liquid in (1) was quite cloudy ; that in (3) was opalescent, though less so than (1). The rest were clear. Twenty-four hours later (4) had become opalescent, (2), (5) and (6), continued clear.

These experiments, and others like them, show that all the cinchona alkaloids possess antiseptic virtues, and that the quantitative difference between them is not very marked. Arranging them in the order of their efficacy, quinia and quinidine stand in the first rank ; cinchonidine next ; and last, cinchonia.

It is interesting to note that of the four alkaloids, cinchonia is the only one which is not fluorescent. Is this physico-chemical property correlated in any way with antiseptic power? The following experiment with *æsculin*, the active principle of the horse-chesnut bark, shows that it is not; or, at any rate, that a substance more fluorescent than quinia possesses no antiseptic virtue comparable with that of the cinchona alkaloids.

EXPERIMENT 10.—Four tubes were charged as follows:—

1. Cohn's solution, 2·5 c.c.; distilled water, 2·5 c.c.
2. Cohn's solution, 2·5 c.c.; distilled water, 1·5 c.c.; solution of *æsculin* (·5 per cent., dissolved in distilled water with the aid of a gentle heat), 1 c.c.; (proportion of drug, ·1 per cent.).
3. Cohn's solution, 2·5 c.c.; distilled water, 0; *æsculin* solution, 2·5 c.c.; (proportion, ·25 per cent.).
4. Cohn's solution, 2 c.c.; *æsculin* solution, 3 c.c.; (proportion of drug, ·3 per cent.).

The liquid in the three last tubes was brilliantly fluorescent, clear, and neutral to test-paper. They were all inoculated with a drop of milky Cohn and plugged with cotton-wool. Three days after, the test-liquid in all four tubes was equally cloudy, and coated with a tenacious pellicle.

A series of experiments was now instituted with several substances to which antiperiodic properties have been ascribed on good authority, though not by any means established to the satisfaction of all clinical observers. The substances chosen were hydrochlorate of berberin, sulphate of beberia, sodic sulphite, sodic hyposulphite, potassic picrate.

Berberin is a bitter alkaloid contained in the barberry and the root of *Jateorrhiza Calumba*; its hydrochlorate is of a brilliant yellow colour, and sparingly soluble in water. Antiperiodic properties have been ascribed to it, though some trials made with it in the wards of King's College Hospital by Dr. Garrod yielded negative results. The following experiments show that its antiseptic power, if it have any, is below that of the cinchona alkaloids:—

EXPERIMENT 11.—A minute cube of egg-albumen was dropped into a tube and covered with 4 c.c. of distilled water. Another tube received a similar bit of albumen, which was covered with

4 c.c. of a .25 per cent. solution of the berberin salt in distilled water; its reaction was neutral. Both tubes were inoculated with pea-water, and plugged with cotton-wool. Four days later, the liquid in both tubes was equally opalescent, and swarmed with extremely minute specimens of *Bacterium termo*.

EXPERIMENT 12.—Two tubes received 2.5 c.c. of Cohn's solution each. To the first was added 2.5 c.c. of distilled water; to the second, 2.5 c.c. of a solution of berberin hydrochlorate (.25 per cent.). Both were inoculated with milky Cohn and stoppered. In three days the liquid in both tubes was equally cloudy and coated with a pellicle of *Zooglaea*.

Beberia is an alkaloid contained in the bark of the green-heart tree (*Nat. Ord. Lauraceæ*). The evidence in behalf of its antiperiodic efficacy is tolerably strong; apart from its use as a febrifuge in British Guiana, Dr. MacLagan of Edinburgh, M. Becquerel of Paris, and others, have testified in its favour. Dr. Garrod ("Essentials of Mat. Medica") is more cautious, giving it credit for antiperiodic power, but adding that "it has been found but an imperfect substitute for quinine." The sulphate is in dark-red scales, and cannot be crystallized; it is probable that it contains more than one principle; it is freely soluble in water, forming a dirty-brown solution which yields a permanent lather on agitation; the solution has to be filtered before use, to free it from suspended impurities; after filtration, it is clear and of a greenish-yellow hue.

EXPERIMENT 13.—Each of two tubes received a small die of egg-albumen: to the first, 4 c.c. of distilled water were added; to the second, 4 c.c. of a .25 per cent. solution of sulphate of beberia. Both were then inoculated with pea-water. Three days after, the liquid in the first tube was quite opalescent, that in the second remaining clear; after the lapse of a week, it was found to contain a small number of extremely minute microzymes.

EXPERIMENT 14.—Four tubes were charged as follows:—

1. Cohn's solution (double strength), 3 c.c.; distilled water, 3 c.c.

2. Cohn's solution, 3 c.c.; distilled water, 1.5 c.c.; solution of sulphate of beberia (1 per cent.), 1.5 c.c.; (proportion .25 per cent.).

3. Cohn's solution, 3 c.c.; distilled water, 2 c.c.; *beberia* solution, 1 c.c.; (proportion, '166 per cent.).

4. Cohn's solution, 3 c.c.; distilled water, 2.5 c.c.; *beberia* solution, .5 c.c.; (proportion, '083 per cent.).

They were all inoculated with milky Cohn and plugged as usual. In four days' time, the liquid in the first tube was quite milky and coated with scum, that in the other tubes remaining perfectly clear. On the seventh day, the liquid in (4) was faintly opalescent, and contained a considerable number of active microzymes; (2) and (3) were still clear. On the tenth day, the contents of these also had become opalescent, but were not coated with a pellicle.

These trials seem to indicate that *beberia* stands on a different footing from *berberin*, approaching and rivalling the cinchona alkaloids in antiseptic power. There is therefore some ground for the hope that it may be found useful in the treatment of septic conditions of the blood, apart from the antiperiodic qualities ascribed to it on clinical evidence.

The sulphites, brought prominently into notice some years ago by Professor Polli, of Milan, have been credited with antiseptic and antiperiodic properties of a high order. They have been employed in the treatment of *ague* and malarious remittents, in that of *pyæmia* and the continued fevers. And though they have not made much way in this country, their claims are supported by too large a mass of documentary evidence to be wholly set aside. This evidence is undoubtedly vitiated by the circumstance that some at least of their advocates are strenuous upholders of the fungoid nature of the paludal miasm; a view which cannot be said to have been refuted, but which has the balance of pathological, geographical, and clinical experience very decidedly against it. Dr. Giovanni Faralli has furnished the latest contribution to this discussion¹. "His opinion in favour of the use of the sulphites and hyposulphites in the treatment of intermittent fever is not very strong." He is against the zymotic theory of this disease. His conclusion

¹ "On the Sulphites and Hyposulphites in the treatment of Intermittent Fever," 1872. Reviewed in the *Brit. and For. Med.-Chir. Review* for October 1873.

seems to be that the sulphites and hyposulphites are really of some use, but that they are far below quinine and arsenic in antiperiodic power. It is a singular fact that he admits the antiseptic power of sodic sulphite, while denying that of quinia; and the superior therapeutic efficacy of the latter in the treatment of ague is one motive for his rejection of the zymotic theory of paludal infection. Putting all pathological hypothesis aside, this statement as to the relative antiseptic power of the sulphites and quinia will not be found to agree with the result of the following experiments:—

EXPERIMENT 15.—Five tubes were charged as follows:—

1. Cohn's solution (quadruple strength), 2 c.c.; distilled water, 6 c.c.

2. Cohn's solution, 2 c.c.; distilled water, 3 c.c.; solution of sodic sulphite (10 per cent.), 3 c.c.; (proportion of drug, 3.75 per cent.).

3. Cohn's solution, 2 c.c.; distilled water, 0; sulphite solution, 6 c.c.; (proportion, 7.5 per cent.).

4. Cohn's solution, 2 c.c.; distilled water, 3 c.c.; solution of sodic hyposulphite (10 per cent.), 3 c.c.; (proportion, 3.75 per cent.).

5. Cohn's solution, 2 c.c.; distilled water, 0; hyposulphite solution, 6 c.c.; (proportion, 7.5 per cent.).

Each of the tubes was thus made to contain 8 c.c. of liquid, and the proportion of nutrient salts was the same in each. The solution of the hyposulphite employed was quite clear; that of the sulphite was slightly milky; but the test-liquids all became clear after standing for half an hour. They were all inoculated with pea-water. Three days later, the liquid in (1) and (4) was very milky, and coated with a thick scum; that in (2) and (5) was opalescent, but had no scum; (3) was clear and continued clear for a week. The result of inspection was confirmed by microscopical examination.

The above experiment shows that sodic hyposulphite, in the proportions employed, exerts no appreciable influence on the reproduction of microzymes; that the sulphite does undoubtedly check or even arrest their evolution; but that its antiseptic power is very far behind that of the cinchona alkaloids or the sulphate of beberia; 3.75 per cent. of the sulphite failing to

accomplish the task easily performed by .25 to .5 per cent. of any of the latter salts.

The salts of picric or carbazotic acid have been repeatedly suggested as substitutes for quinia in the treatment of the paludal fevers. Only a year ago, M. Dujardin-Beaumetz, published some cases of ague treated with the picrate or carbazotate of ammonia.¹ Four quotidians and one tertian, which had already failed to yield to quinia, were permanently cured in from four to eight days by doses varying from one to eight centigrammes (.15 to 1.2 grains) daily in pills, and this without causing yellowness of the skin, digestive or nervous troubles. Such records would have to be extended and multiplied before a final judgment could be pronounced; but there are facts enough to justify us in placing the salts of picric acid provisionally among antiperiodic remedies. I am not aware that the antiperiodic power of the picrates has been associated with their antiseptic qualities: or that they have ever been given in cases of septicæmia, or recommended for their antiseptic virtues.

EXPERIMENT 16.—Five tubes received 8 c.c., each of Cohn's solution. They were medicated as follows:—

1. Nil
2. .25 per cent. of quinia hydrochlorate.
3. .25 per cent. of potassic picrate.
4. .375 per cent. of potassic picrate.
5. .4 per cent. of potassic picrate.

They were all inoculated with pea-water and stoppered. Three days afterwards (1) was quite cloudy and coated with a pellicle. The rest were clear. A week later, (2) was teeming with microzymes, while (3), (4), and (5) remained clear.

EXPERIMENT 17.—Five tubes were charged as follows:—

1. Cohn's solution (quadruple strength), 2 c.c.; distilled water, 6 c.c.
2. Cohn's solution, 2 c.c.; distilled water, 2 c.c.; solution of potassic picrate (.5 per cent.), 4 c.c.; (proportion of drug, .25 per cent.).
3. Cohn's solution, 2 c.c.; distilled water, 3 c.c.; picrate solution, 3 c.c.; (proportion .187 per cent.).

¹ See abstract in the *Centralblatt* for November 1872, taken from the *Gazette Médicale*.

4. Cohn's solution, 2 c.c.; distilled water, 4 c.c.; picrate solution, 2 c.c.; (proportion '125 per cent.).

5. Cohn's solution, 2 c.c.; distilled water, 5 c.c.; picrate solution, 1 c.c.; (proportion '062 per cent.).

All were inoculated with pea-water and plugged. Four days afterwards (1) was milky and coated with scum; (2), (3), and (4) were clear; (5) was slightly opalescent. At the end of a week the only change observed was a slight increase in the cloudiness of (5).

EXPERIMENT 18.—Five tubes were charged thus:—

1. Beef-tea, 1 c.c.; distilled water, 7 c.c.

2. Beef-tea, 1 c.c.; distilled water, 3 c.c.; solution of potassic picrate ('5 per cent.), 4 c.c.; (proportion '25 per cent.).

3. Beef-tea, 1 c.c.; distilled water, 5 c.c.; solution of the picrate, 2 c.c.; (proportion '125 per cent.).

4. Beef-tea, 1 c.c.; distilled water, 0; solution of picric acid ('25 per cent.), 7 c.c.; (proportion '22 per cent.).

5. Beef-tea, 1 c.c.; distilled water, 3 c.c.; picric acid solution, 4 c.c.; (proportion '125 per cent.).

All were inoculated with pea-water. A week afterwards (1) (2) and (3) were swarming with microzymes, (4) and (5) remained clear.

Experiments 16 and 17 show that potassic picrate is equal or perhaps even superior to quinia in antiseptic power, when Cohn's solution is employed as the test-liquid. Its efficacy is markedly diminished (18) when beef-tea is substituted for the artificial test-liquid. The latter experiment is recorded in order to illustrate the fact that the nature and composition of the test-liquid employed influences the result of the experiment very materially; and that it would be unsafe to draw quantitative inferences from experiments performed with different test-solutions. The comparative value of the individual experiment is not thereby impaired. The immunity of the tubes numbered (4) and (5) in 18 is accounted for by the acid reaction of their contents, and says nothing for the specific influence of picric acid.

In estimating the relatively high rank taken by potassic picrate in these experiments, it must not be forgotten that the doses of this salt capable of being given without risk are

probably below those of quinia. I do not know that we are in possession of any data as to the maximum dose of the carbazotates.

The antiseptic value of the sulphocarbolates has been strongly insisted on by Dr. Sansom in his writings on the antiseptic system.¹ They have not, so far as I am aware, been credited with antiperiodic virtues. The following experiments were designed to bring sodic sulphocarbolate into comparison with *beberia* :—

EXPERIMENT 19.—A die of egg-albumen was introduced into each of three tubes : (1) was covered with 4 c.c. of a .25 per cent. solution of sodic sulphocarbolate ; (2) with 4 c.c. of a .25 per cent. solution of sulphate of *beberia* ; (3) with the same amount of distilled water. In three days' time (1) and (3) were equally opalescent ; (2) remained quite clear.

EXPERIMENT 20.—Four tubes were charged as follows :—

1. Cohn's solution (double strength), 3 c.c. ; distilled water, 3 c.c.

2. Cohn's solution, 3 c.c. : distilled water, 1.5 c.c. ; solution of sulphate of *beberia* (1 per cent.), 1.5 c.c. ; (proportion of drug, .25 per cent.).

3. Cohn's solution, 3 c.c. ; distilled water, 0 ; solution of potassic picrate (.5 per cent.), 3 c.c. ; (proportion, .25 per cent.).

4. Cohn's solution, 3 c.c. ; distilled water, 1.5 c.c. ; solution of sodic sulphocarbolate (1 per cent.), 1.5 c.c. ; (proportion, .25 per cent.).

They were all inoculated with milky Cohn, and plugged as usual. Five days afterwards (1) was milky and coated with a pellicle ; (2) and (3) were quite clear ; (4) was faintly opalescent, and free from scum ; under the microscope it was seen to contain a number of excessively minute microzymes, moving about actively.

EXPERIMENT 21.—Four tubes were charged as follows :—

1. Cohn's solution (double strength), 3 c.c. ; distilled water, 3 c.c.

2. Cohn's solution, 3 c.c. ; distilled water, 2 c.c. ; solution of sodic sulphocarbolate (2 per cent.), 1 c.c. ; (proportion, .333 per cent.).

¹ *Practitioner* for June 1872.

3. Cohn's solution, 3 c.c. ; distilled water, 1 c.c. ; sulphocarbonate solution, 2 c.c. ; (proportion, '666 per cent.).

4. Cohn's solution, 3 c.c. ; distilled water, 0 ; sulphocarbonate solution, 3 c.c. ; (proportion, 1 per cent.).

They were all inoculated as in the foregoing experiment. Four days later (1) was milky, and coated with scum. The rest were quite clear. After seven days (2) was milky, (3) opalescent, while (4) continued clear.

These experiments indicate that sodic sulphocarbonate is endowed with a considerable degree of antiseptic power, though decidedly below that of the cinchona alkaloids or the sulphate of beberia. It seems likely, *a priori*, that it would prove inferior to the latter agents in the treatment of septic states of the blood, if administered in equal doses ; it may well be, however, that larger proportions of the sulphocarbolates would be tolerated by the system than of the vegetable principles.

Of the various substances experimented on by Professor Binz (*l. c.*), strychnia was found by him to stand next to quinia, in antiseptic power. The question is not one of great practical interest, inasmuch as the effect of strychnia upon the nervous centres would always preclude its administration in doses adequate to produce either antiseptic or antiperiodic effects in the body. The following experiment corroborates Professor Binz's statement :—

EXPERIMENT 22.—Five tubes were charged as follows :—

1. Cohn's solution (double strength), 3 c.c. ; distilled water, 3 c.c.

2. Cohn's solution, 3 c.c. ; solution of potassic picrate ('5 per cent.), 3 c.c. ; (proportion, '25 per cent.).

3. Cohn's solution, 3 c.c. ; distilled water, 1'5 c.c. ; solution of sulphate of beberia (1 per cent.), 1'5 c.c. ; (proportion, '25 per cent.).

4. Cohn's solution, 3 c.c. ; distilled water, 1'5 c.c. ; solution of nitrate of strychnia (1 per cent.), 1'5 c.c. ; (proportion, '25 per cent.).

5. Cohn's solution, 3 c.c. ; distilled water, 0 ; strychnia solution, 3 c.c. ; (proportion, '5 per cent.).

All the tubes were inoculated with milky Cohn and plugged with cotton-wool. After the lapse of seven days (1) was milky

and coated with scum ; (2) and (3) were clear ; (4) was faintly opalescent ; (5) quite clear.

Strychnia is thus seen to stand very near quinia, beberia, and potassic picrate in antiseptic power. The action of the quinia salts on microzymes is often brought under the general statement that these salts are "protoplasmic poisons."¹ In view of this generalisation and of the fungoid character assigned to bacteria by the most competent authorities, it seemed desirable to test the effect of the cinchona alkaloids on the growth of *Penicillium*.

EXPERIMENT 23.—Five large tubes were charged with Cohn's solution, to which 15 per cent. of cane-sugar had been added. The first tube was not medicated ; the other four tubes received 166 per cent. each of the hydrochlorates of quinia, cinchonina, cinchonidine, and quinidine. *Penicillium*-spores were sown in all. A week later the surface of the liquid in all the tubes was coated with a greenish scum, which was equally luxuriant in all. The subjacent liquid was clear, and free from microzymes.

EXPERIMENT 24.—Four tubes were charged with the same solution as in the foregoing experiment. These were medicated as follows :—

1. Quinia hydrochlorate, 75 per cent.
2. Cinchonina, 75 per cent.
3. Cinchonidine, 75 per cent.
4. Quinidine, 75 per cent.

The liquid in all the tubes was acidulated with HCl dil., which helped to dissolve the alkaloids, and favoured the growth of the fungus. A few *penicillium*-spores were sown in each tube ; the tubes were then plugged with cotton-wool, and put in a warm place. In nine days' time the liquid in all was thickly coated with a puckered greenish-yellow scum. The subjacent liquid continued clear.

The following comparison was then instituted between sodic sulphite and sodic hyposulphite.

EXPERIMENT 25.—Two tubes were charged with the same solution as in the previous experiment. The first received 1 per cent. of sodic sulphite ; the second the same proportion of sodic

¹ Ringer, "Handbook of Therapeutics."

hyposulphite. *Penicillium*-spores were sown in both, and a week after had vegetated luxuriantly in both.

EXPERIMENT 26.—Three tubes were charged with the same solution as before.

1 contained 7·5 per cent. of sodic hyposulphite.

2 contained 7·5 per cent. of sodic sulphite.

3 contained 75 per cent. of mercuric chloride.

The liquid in all the tubes was then acidulated with HCl dil. In (1) a precipitate of free sulphur was thrown down. They were then sown with *penicillium*-spores. Ten days afterwards (2) was coated with a thick scum, while no germination had occurred in either (1) or (3).

These experiments seem to show that the cinchona alkaloids, in greater proportion than is needful to arrest the multiplication of microzymes, exert no appreciable influence on the growth of *Penicillium*. That the same proportion of mercuric chloride renders the spores absolutely barren. That sodic sulphite and hyposulphite are without any effect; but that the latter, in acid solutions, yielding free sulphur and sulphur dioxide, appears to check the development of the spores.

Quinia, as an antiseptic, has thus been compared with:—

1. Mercuric chloride, sodic sulphocarbonate, and strychnia—substances to which antiseptic properties are ascribed.

2. Berberin, beberia, picrate of potassium, quinidine, cinchonia, cinchonidine, æsculin—substances credited with antiperiodic virtues.

3. Sodic sulphite and hyposulphite—to which both antiseptic and antiperiodic properties are attributed.

The following thoughts are suggested by the results of this comparison:—

1. Quinia is doubtless excelled by other antiseptics, but there is no substance equal to it in antiseptic power which can be introduced into the blood in the same proportions without risk of fatal effects, if we except the other cinchona alkaloids and the sulphate of beberia.

2. Quinia, in such fractional doses as are capable of being introduced into the circulation, exerts an inhibitory, not a toxic, action upon microzymes. It may check septic changes; it cannot destroy the organisms to which such changes are due.

3. The four cinchona alkaloids are very nearly equal in antiseptic power. Arranged in the strict order of their efficacy, they stand thus: Quinia = quinidine; next comes cinchonidine; last, though at no great distance, cinchonina.

4. It is a singular circumstance that this order corresponds to that in which the four alkaloids are arranged in the Report of the Madras Cinchona Commission in 1868, with reference, not to their antiseptic, but to their *antiperiodic* power, as determined by clinical experience. This report¹ establishes:

- (a.) That quinidine equals quinia in febrifuge action.
- (b.) That cinchonidine is only slightly less efficacious.
- (c.) That cinchonina, though somewhat inferior, is, notwithstanding, a valuable remedial agent in fever.

This agreement may possibly be due to coincidence; if not, it seems to afford some countenance to the zymotic theory of the paludal miasm; or it would perhaps be more correct to say that it does not *oppose* it. It really shows no more than that the one property is in some way correlated with the other; it does not prove their identity.

5. Among reputed antiperiodics, the sulphate of beberia seems to equal quinia in antiseptic power.

6. Among reputed antiseptics, sodic sulphocarbolate and strychnia have a decided value, though they stand some way below quinia.

7. Sodic sulphite has a feeble, though decided, antiseptic power; sodic hyposulphite little or none.

8. Berberin and æsculin are hardly, if at all, antiseptic.

9. Potassic picrate is a strong antiseptic, almost, if not quite, equal to quinia. It is doubtful, however, whether it can be administered in sufficient doses without danger to life.

10. The action of inhibitory drugs on the vitality of microzymes affords no clue to their action on *Penicillium*. Substances like mercuric chloride, which are immediately fatal to the former, arrest the growth of the latter when employed in the same minute properties.

¹ *Lancet*, May 24, 1873.

II. THE ACTION OF THE CINCHONA ALKALOIDS AND SOME OF THEIR CONGENERS ON THE COLOURLESS BLOOD CORPUSCLES.

THE toxic action of quinia on the colourless corpuscles of the blood was first noticed by Professor Binz; and his observations have since been extended and corroborated by other investigators. This effect of the alkaloid acquires a special significance in view of recent doctrines of inflammation and suppuration; the emigration of leucocytes from the vessels into the inflamed tissue giving rise to the whole, or a varying fraction of the total inflammatory exudation. Any substance capable of arresting or retarding the movements of the corpuscles might therefore be expected to arrest or retard the local consequences of tissue-irritation. And it is probable that the proliferative activity of the autochthonous corpuscular elements, depending as it does on properties common to them with the white corpuscles of the blood, may be restrained by the same means that are found effectual in checking the migratory propensities of the latter.

That quinia really does possess such antiphlogistic powers is shown on the one hand by the clinical experience of many years, on the other by the fact that the migration of leucocytes into the exposed mesentery of the frog (Cohnheim's experiment) may be checked or wholly arrested by the introduction of a minute dose of quinia into the current of its circulation, (Binz, *l. c.* p. 37). The study of the effects produced by the alkaloid on the spontaneous movements of the blood-corpuscles, as seen on the stage of the microscope, does accordingly appear to shed a considerable light on one of its therapeutic applications.

It seemed desirable to ascertain if the other cinchona alkaloids were endowed with similar properties in an equal degree. Further, to compare this action of quinia with that of some other substances resembling it in its antiseptic or antiperiodic properties, with a view to learning whether the latter qualities were necessarily correlated with its power of arresting protoplasmic movement, or only incidentally associated therewith. With this end in view, comparative trials were made with the four cinchona alkaloids, sulphate of beberia, nitrate of strychnia, and potassic picrate.

The blood-corpuscles of the newt were employed throughout. The method of investigation differed somewhat from that followed by Geltowsky in his interesting experiments with quinia.¹ The drugs employed were dissolved in a .75 per cent. solution of sodic chloride. A large drop of the solution having been placed on a slide, the cut surface of the newt's tail was touched with a clean cover-glass ($\frac{1}{8}$ inch in diameter) ; a few seconds having been permitted to elapse (to allow of coagulation beginning) the cover-glass was carefully inverted on the drop. The excess of fluid was then drained from the edges of the preparation with bibulous paper, and a ring of oil painted round it to prevent evaporation. The following are the chief advantages of this method :—

1. The proportion of blood to the quantity of solution employed being almost infinitesimally small, it need not be taken into account in estimating the percentage composition of the latter.

2. The film of blood being extremely thin, and coagulation only just beginning as it is brought into contact with the solution, the perfect intermixture of the two is rendered certain. A drop of newt's blood removed with a pipette, sets into a small cylindrical clot before it can be transferred to the slide, and it is then difficult to make sure that the solution is really brought into contact with each individual corpuscle.

3. The comparatively small number of leucocytes in the preparation enables the observer to examine each individual one within a period of five minutes or less.

4. The use of salt solution instead of water for dissolving the drug obviates all risk of an accident which occurs repeatedly when water is employed as the solvent, viz. the decolorisation of the red corpuscles and the destruction of the colourless ones. Minor degrees of this change are inevitable, even when very small proportions of the liquid are used ; with the salt solution, no risk is run even when a relatively enormous excess of liquid is added to the blood, since it produces no appreciable effect either on the red or on the colourless elements. In some of the following experiments, the migratory activity of the leucocytes was actively displayed after the lapse of thirty-six hours, and the red corpuscles were not perceptibly altered.

¹ *Practitioner*, June 1872. —

It is important to define strictly what is meant by the term "migratory movement" as used in the following pages. If a drop of newt's blood be prepared with salt solution in the manner just described, and examined some hours after, the following will be among the appearances presented by the colourless corpuscles.

1. Migratory movement. The protoplasmic mass is spread out into a thin film, adherent to the glass, and of a most irregular shape. The outline of the film may be well-marked in parts, but is never complete; the blurred portions being beset with fine filamentous processes, which are continually changing in size and number, some being retracted, while others are put out. The aggregate result of the changes of form which the corpuscle undergoes is a change in its *position*; it migrates from one part of the field to another.

2. Change of form without change of position.

a. The corpuscle is spherical, non-adherent, its surface velvety or shaggy, giving it an indistinct or blurred outline.

β. Its outline is sharp; it is more or less spherical, non-adherent, and exhibits a peculiar waxy lustre. Its nucleus or nuclei are usually invisible. If carefully watched, it may be seen to change its shape very gradually; one part of its surface rising slowly into knob-like protuberances, while another part sinks in to a corresponding extent: the protuberances being limited by definite contours, and exhibiting the same waxy lustre as the main body of the corpuscle. No filamentous processes are put forth. Such corpuscles, though their movements are undoubtedly vital, do not change their place. But they are capable of resuming the migratory condition as soon as the cause of their temporary quiescence ceases to operate.

γ. The corpuscle may be surrounded by projections of another kind. The main body shrinks gradually into an irregular lump, while minute, translucent spherules of various sizes present themselves round its edge. These spherules do not appear to consist of the same substance as the main bulk of the corpuscle: they may even become detached from it, and float away. The appearances are those which might be presented by a coagulum slowly shrinking and expelling a clear fluid from its interstices;

the fluid not being miscible with that in which the corpuscle is suspended. Whether a leucocyte in this state may, as in the two foregoing conditions, resume its migratory powers, I am unable to state.

3. Permanent repose or death. The corpuscle is spherical or spheroidal, with a distinct outline. It does not exhibit any sort of movement; its protoplasm is very granular, and its nuclei well-marked. This condition is irremediable, and is the immediate precursor of disintegration.

The first of these conditions is that of the highest vital activity; the second may pass either into the first or into the third. It is possible so to graduate the dose of a poisonous agent, as to obtain the second or the third condition at will. In the following experiments, the exact state of the colourless corpuscles is usually indicated: the discordant results of previous observers in the case of quinia being probably due to a neglect of this exactitude. The condition of the red discs is often noted, inasmuch as they furnish the most delicate test of the neutrality of the liquid in which they float—the faintest trace of acid causing the delimitation, a larger proportion the granulation, of their nuclei.

The drugs employed were the hydrochlorates of the four cinchona alkaloids, potassic picrate, æsculin, sulphate of beberia, and strychnia nitrate. The solutions were all neutral to test-paper. The temperature of the room varied from 65° to 75° Fahr.

No.	Drug.	Proportion.	Remarks.
27	Quinia hydrochlorate	1 in 700	In ten minutes no movement.
28	Quinidine hydrochlorate	1 in 700	In fifteen minutes the majority quiescent. No migratory movement. Three waxy and mobile (28); their movements ceased in forty minutes.
29	Cinchonia hydrochlorate	1 in 700	In ten minutes no movement. Red corpuscles unaltered.
30	Cinchonidine hydrochlorate	1 in 700	In fifteen minutes no movement.
31	Cinchonidine hydrochlorate	1 in 700	In ten minutes no migration; several waxy and mobile (28); they came to rest within an hour.

No.	Drug.	Proportion.	Remarks.
32	NaCl	75 per cent.	Active migration going on during during the six hours the observation lasted.
33	Quinia hydrochlorate	1 in 1,000	In ten minutes all motionless.
34	Quinidine hydrochlorate	1 in 1,000	In thirty minutes all motionless.
35	Cinchonia hydrochlorate	1 in 1,000	In fifteen minutes no movement.
36	Cinchonidine hydrochlorate	1 in 1,000	In fifteen minutes no movement.
37	Quinia hydrochlorate	1 in 1,200	In fifteen minutes no migratory corpuscles to be seen : several waxy and mobile. In forty-five minutes all at rest.
38	Cinchonia hydrochlorate	1 in 1,200	In fifteen minutes no migration. Changes of form continued for one hour.
39	Quinia hydrochlorate	1 in 1,500	In ten minutes no migratory forms to be detected. Many are waxy with actively moving protuberances. In one hour thirty-five minutes, two only are moving feebly. In six hours many are granular, some waxy, all at rest.
40	Quinidine hydrochlorate	1 in 1,500	In twenty minutes several are migrating actively ; most are waxy and mobile. In fifty minutes none migrating ; most are granular, a few waxy, a few in the state described under 2γ. In two hours no movement.
41	Quinidine hydrochlorate	1 in 1,500	In twenty minutes no migration. Waxy protuberances very active. In fifty minutes most are spheroidal and waxy, with 2-4 large, obvious nuclei. No movement. In two hours some granular, most waxy, all at rest. In five hours all dead and granular.
42	NaCl	75 per cent.	Migratory movements actively proceeding after the lapse of twenty-two hours.
43	Cinchonia hydrochlorate	1 in 1,500	In twenty minutes all motionless.
44	Cinchonia hydrochlorate	1 in 1,500	In twenty-five minutes two migrating feebly; knobs very active. In sixty minutes no migration ; waxy and very mobile. Two hours ten minutes, the same. Two hours forty-five minutes many spherical and at rest ; many still moving. Four hours five minutes, all at rest.
45	Cinchonidine hydrochlorate	1 in 1,500	In ten minutes a large number migrating ; forty minutes, many waxy, many migrating ; one hour, none migrating, waxy ones very mobile ; one hour thirty-five minutes, many granular, many waxy, no migration ; two hours ten minutes, many still waxy.
46	Cinchonidine hydrochlorate	1 in 1,500	In twenty minutes no migration. Waxy protuberances very active. One hour, most spherical, at rest ; some still moving. Two hours twenty minutes, no change.

The above experiments seem to show that the four cinchona alkaloids are able speedily to arrest the *migratory* movement of the colourless corpuscles of newt's blood, in the proportion of 1 in 1500. That the quantitative differences between them are not well-marked: quinia appearing to stand first in order of power. That the proportion of the alkaloid necessary to arrest the movement of the waxy protuberances (2β), is much larger than that which arrests migration and the putting forth of filamentous processes. And this would lead us to expect that it may be possible to check the migratory propensities of the colourless corpuscles while in the body, without impairing, or permanently abolishing, their vitality—that they may be narcotized, so to speak, without being killed.

No.	Drug.	Proportion.	Remarks.
47	Sulphate of Beberia	1 in 500	In ten minutes the majority are waxy and mobile. None migrating. (The nuclei of the red corpuscles are more defined than usual, without being in the least granular. No trace of acidity in the solution can be detected by test-paper. This delimitation of nuclei is seen in all the preparations containing beberia, even when the proportion of the drug is 1 in 1500.) Thirty minutes, no migration; movements active. One hour thirty-five minutes, all at rest.
48	Sulphate of Beberia	1 in 700	In fifteen minutes none migrating; many spherical; many waxy, changing their form. Two hours, protuberances active; no migration. Ten hours, all motionless; mostly granular.
49	Sulphate of Beberia	1 in 1,000	In fifteen minutes no migration. Three hours thirty minutes, protuberances lively. Ten hours, all dead and granular.
50	Sulphate of Beberia	1 in 1,500	In ten minutes no migration. One hour protuberances lively; no migratory forms. Ten hours, all at rest and granular.
51	Sulphate of Beberia	1 in 1,500	In fifteen minutes no migration. Forty-five minutes, waxy; moving actively. Two hours most spherical, a few still moving. Three hours forty-five minutes the same. Five hours, all at rest.
52	Strychnia nitrate	1 in 500	In thirty minutes none migrating; many spherical and quiescent, though not granular. Three hours forty-five minutes, many waxy and mobile; no migratory or shaggy forms. Five hours thirty minutes, the same. Twenty-four hours, many waxy and mobile; none migrating.

No.	Drug.	Proportion.	Remarks.
53	Strychnia nitrate	1 in 1,500	In twenty-five minutes several migrating; many waxy. One hour forty minutes, three migrating actively; most are quiescent. Three hours forty minutes, several migrating. Five hours twenty minutes, two migrating sluggishly; most waxy. Twenty-four hours, two still migrating; many shaggy; many waxy and changing form. The red disks are unaltered.
54	NaCl	75 per cent.	Migratory movements seen to continue for thirty-six hours.

Sulphate of beberia, which approaches the cinchona alkaloids so closely in antiseptic power, is thus seen to rival them in its inhibitory action upon the *migratory* movement of the colourless corpuscles. The effect of its solutions in defining the nuclei of the red discs—an effect not produced by the cinchona alkaloids—leads one to suspect that the mode of action is somewhat different in the two cases. Strychnia has some influence upon the colourless corpuscles; but it stands very far behind beberia and the cinchona alkaloids.

No.	Drug.	Proportion.	Remarks.
55	Potassic Picrate	1 in 1000	In two hours fifteen minutes, several corpuscles are still actively migrating. The red disks are unaltered.
56	Potassic Picrate	1 in 700	In one hour several are in active migration. Two hours fifteen minutes, two corpuscles still migrating, though sluggishly.
57	Potassic Picrate	1 in 500	In fifteen minutes three are migrating; many are waxy and changing form. Forty-five minutes, no migration; protuberances still active.
58	Potassic Picrate	1 in 400	In twenty minutes no movement.
59	Potassic Picrate	1 in 400	In twenty-five minutes no migration; many waxy and moving. Fifty-five minutes, most are spherical, with well-defined double or triple nuclei; some waxy; no migration; red disks unaltered. One hour fifty minutes, almost all in permanent repose.

These experiments seem to show that potassic picrate, standing almost on a level with the cinchona alkaloids in antiseptic power,

exerts a comparatively feeble influence over the spontaneous movements of the colourless corpuscles. It cannot, therefore, be asserted, at present, that the antiseptic and the antiphlogistic actions of a drug are necessarily connected with each other.

The following experiments with *æsculin* prove that it exerts no very marked effect on the migratory activity of the blood-corpuscles.

No.	Drug.	Proportion.	Remarks.
60	<i>Æsculin.</i>	1 in 500	In thirty minutes several shaggy (2a). Two hours many shaggy; none actually migrating. Five hours thirty minutes, many waxy and changing form; no migration.
61	<i>Æsculin.</i>	1 in 900	In twenty minutes, many migrating; several waxy. Five hours thirty minutes no change.
62	<i>Æsculin.</i>	1 in 1500	In fifteen minutes numbers migrating. Six hours, the same.

CLINICAL NOTES ON CERTAIN SKIN DISEASES AND THEIR TREATMENT.

BY EDWARD B. GRAY, M.D.

ECZEMA.—In 1864-5 an undergraduate, aged 20, consulted me occasionally, on account of a moist eczema, shifting about the forehead, but showing a preference for one or other supra-orbital region. I could never find anything wrong in his general health or habits. After repeated improvements and relapses, the eruption finally got well, having lasted in all some eighteen months. I thought little of the case at the time, except so far as it baffled my efforts to cure it. I have thought much of it since, on account of the patient's remarkable personal and family history, which has latterly become known to me bit by bit.

His eczema supervened slowly on the subsidence of spasmodic asthma, which had troubled him very frequently from early boyhood. For the first twelve years of his life he had pretty constant nocturnal incontinence of urine. He is now in perfect health, with one exception—that when over-tired, or worried, he gets rather severe clavus-headache on one side.

So much for his own medical history; now for that of his family. A brother stammered badly from childhood up to 23 or 24 years of age. A sister, till past puberty, was martyr to severe spasmodic asthma: after the subsidence of the asthma she had from time to time for many years a rough scaly condition of the skin of the face (chiefly the forehead), and occasional gastralgia of unquestionable neuralgic type. His mother, now advanced in years, suffers at times from infra-orbital and parietal neuralgia, culminating in a sort of erysipelatous inflammation of

the skin of the affected parts. His paternal uncle was all his life an imbecile, and died between 60 and 70 of general paralysis.

The sequence of events in this patient's case, read in the light of his family history, makes it, to my mind, in the highest degree probable that his frontal eczema, supervening on asthma, was simply a "transmutation of neurosis" (Trousseau) from his vagus to his trigeminus nerve. By *simply* I mean, that both the skin disease and the asthma were the expression, not of any so-called blood diathesis, but of some fault (probably of nutrition) in that small central brain-tract, in which the vagus and trigeminus have common origin. Such an explanation will probably commend itself to those who recognize the agency of the nerve-centres in the production of many localized disturbances of skin-nutrition.

Looking back at my treatment of this case, I fear it was wrong. It was the old-fashioned combination of mild tonics and alteratives. Had the patient's strong neurotic taint been from the first known and kept steadily in view—in other words—had all my efforts been directed to feeding and toning up his nervous system with such remedies as cod-oil and arsenic, his eruption would probably have got well much sooner than it did.

Acute Syphilitic Lichen may so closely simulate the primary stage of small-pox eruption, as to render the diagnosis at first a matter of difficulty. In 1863, Mary H——, aged 22, came from a neighbouring village into the Radcliffe Infirmary, under my care, with moderate swelling and redness of one side of the fauces, and inflammatory enlargements of the glands beneath the corresponding jaw. The throat affection was of three weeks' standing; but she had suffered from general malaise since suppression of the catamenia, three months before. There was nothing in the throat affection, nor in her general condition, to raise any suspicion of syphilis. After three or four days of sharp febrile symptoms, with much pain in back and limbs, an abundant supply of small, hard, red papules appeared, chiefly on face, neck, and forearms. Had small-pox been prevailing at the time, few would, I think, have hesitated in pronouncing this to be a case of it, so closely did the eruption and premonitory symptoms resemble those of variola. As, however, there was no small-pox about, careful search was made for some clue to

the real nature of the eruption. Some hard and rather enlarged glands were now discovered in one groin. Very soon she began to have severe nocturnal pains in both clavicles. The papules desquamated and cleared away, leaving behind them dark coppery stains, so that altogether the syphilitic origin of the eruption seemed beyond reasonable doubt.

Ringworm (tinea tonsurans) in the scalp is often hard to cure, and without very elaborate and repeated microscopic examination, beyond what one can often find time to make, it may be hard to say when it is cured. A child has a patch of ringworm on its scalp. You have applied iodine, perchloride of mercury, or some one of the ordinary parasitocides. At the end, say, of a fortnight, the place looks better, but is still somewhat scurfy, and the skin perhaps thicker than natural. How much of this irritation is due simply to the remedies you have been using, and how much to a still vital remnant of the disease, is now and then a hard, and at the same time a pressing, question to settle; as, for instance, when the disease is keeping a boy away from school, greatly to his parents' disgust and impatience. In pale, delicate children I have many times seen the skin of the tinea-patch remain thick and scurfy, long after repeated and careful microscopic examination had ceased to find any trace of the parasite. In such cases, if the scalp be allowed entire rest from all local treatment, *washing included*, for a week or so, it will probably have got nearly or quite sound. But if without an appeal to the microscope, or dissatisfied with the result of your appeal, and, hoping to be on the safe side, you go on using even the milder parasitocides, the comparatively quiescent site of the tinea is very apt to suddenly blaze out into an augry eczematous or pustular eruption.

Whatever remedy may be used to kill the parasite, I suspect it is good practice to keep the affected portion of skin constantly well greased, for the same reason that one prescribes inunction in scarlatina—*i. e.* to clog the wings of those minute germs of disease, which might otherwise get loose and float in the air, to settle and vegetate where they are not wanted.

A few months back, a schoolgirl was brought to me, with a tinea-patch on her scalp. Very careful search failed to reveal any second nidus of the disease elsewhere, either on head or

body. Tincture of iodine was applied daily to the patch, and she was kept strictly isolated from her schoolfellows. On her coming to see me a fortnight afterwards, spores were still detected in the primary patch, and three other smaller patches, of evident recent date, presented themselves on different parts of the scalp. I have no doubt this was a case of auto-inoculation, the disease most likely having been propagated through the medium of the patient's own hair-brush. Similarly I have seen favus developed on a patient's finger tips from his scratching favus-crusts on his scalp. The possibility of auto-inoculation of parasitic skin-disease is a thing to be borne in mind. I have not yet tested the remedy, but suspect that, in cases of ordinary tinea, some carbolic pomade daily rubbed into the unaffected portion of scalp would render it proof against the invasion of stray spores.

A piece of my experience in a recent obstinate case of multiple scalp-ringworm in a schoolboy may furnish a hint to others. After trying in vain two or three different remedies, I prescribed some Unguent. Sulphuris Iodidi, fortunately adding a caution to use it at first tentatively to a single patch. The second application of it produced not only angry pustulation of the patch it was applied to, but in the course of a week inflammation and suppuration of the adjacent occipital gland.

ON OXIDE OF ZINC IN THE TREATMENT OF DIARRHŒA.

BY WILLIAM BERRY, M.R.C.S. ENGLAND, L.R.C.P. AND S. EDINBURGH.

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OXIDE of Zinc having been recommended by Dr. Brakenridge of Edinburgh, and Dr. Mackey of Birmingham in the treatment of infantile diarrhœa, as may be seen in the *Medical Times and Gazette* of February 15th and the *British Medical Journal* of July 12th, I resolved to give it a fair trial, not only in children, but also in the autumnal diarrhœa of adults. So far I have every reason to be satisfied with it as a remedy for diarrhœa in children, especially in those in which the cause appears to be some irritation of the nerve-centres presiding over the alimentary canal. In adults I have found it useful in cases of lenteric diarrhœa, but not so beneficial as the aromatic chalk powder of the pharmacopœia in ordinary cases.

I think with Dr. Brakenridge that in the majority of cases of diarrhœa in children—though not in all—the nervous system plays an important part; especially in those children who are teething, and in children of the poorer classes who are ill fed and badly clothed.

Whether we accept the theory of Dr. Brakenridge as to the cause of diarrhœa in children or not, there can be no doubt that the beneficial effects derived from the remedy are due to its tonic and astringent properties, I am inclined for my own part to think that its antispasmodic properties have little to do with its efficacy.

That infantile diarrhœa is in a great measure due to a debilitated state of the nervous system and to a hyperæmic condition of the mucous membrane of the bowels, is proved by the readiness with which it is relieved by oxide of zinc.

In the diarrhœa of teething children, and those whose digestion is at fault, the frequency of the evacuations is at once checked, and the character of the motions altered.

The remedy produces nausea in some cases unless a little food be administered just before it, but in many cases no nausea is produced although this point is not attended to.

I here give the notes of a few cases in which the remedy was used with great benefit.

CASE 1.—James H——, aged 1 year, has had tolerably good health up to the time of teething, he is at present (August 5th) “cutting some teeth,” and the last few days has been vomiting and purging several times a day: the motion being somewhat slimy and greenish in colour, ordered 2 grains of oxide of zinc every three hours, given with mucilage and simple syrup, the following prescription being given:—

R. Zinci Oxidi gr: 32
Mucilaginis
Syrup: Simpl: āā ʒij
Aqua ad ʒij
S. ʒj 3tis horis.

August 8th.—Much improved, the motions of a better colour, and only three in the day, rests and takes food better.

August 12th.—Quite well, vomiting and purging quite ceased.

CASE 2.—*August 6th.* Mary F——, aged 1 year and seven months, was seized with vomiting and purging three days ago, purged very frequently, and motions curdy. Ordered 2 grains of oxide of zinc every three hours, in the same manner as in the previous case.

August 8th.—Much better, only four motions in the day.

August 13th.—Quite better.

CASE 3.—John E——, aged 17 months, a delicate-looking child, teething, had been vomiting and purging for about a week. Ordered 1½ grains of oxide of zinc every three hours on August 7th.

August 11th.—Purging less frequent and vomiting ceased.

August 16th.—Purging quite better; the child is much improved in appearance.

CASE 4.—*August 7th.* Alice H——, aged 44 years, was seized with vomiting and purging a week ago, this was checked by some simple remedy, but the purging returned two days ago, and the motions were accompanied with tenesmus and were streaked with blood. She also complained of frontal headache and dizziness. Ordered 5 grains of oxide of zinc every four hours, and 6 grains of Dover's Powder night and morning.

August 14th.—Pain in the head and dizziness gone, purging quite better, only feels weak.

CASE 5.—Isabella C——, aged 2 years, a delicate strumous child, suffering from vaginitis for some time, has had diarrhoea for two days (*August 9th*), vomiting also being present. Ordered 2 grains of oxide of zinc every three hours.

August 14th.—Purging and vomiting better.

August 20th.—Child much improved in health, bowels act naturally.

CASE 6.—*August 12th.* Thomas N——, aged 1 year, a delicate scrofulous-looking child, has been vomiting and purging for a few days. Ordered 2 grains every three hours.

August 18th.—Vomiting and purging better.

CASE 7.—*August 12th.* Joseph A——, aged 9 months, a fine-looking and plump child, has been vomiting and purging for three days. Ordered 1 grain of oxide of zinc every three hours.

August 17th.—Quite better.

CASE 8.—*August 13th.* Isabella F——, aged 1 year, a delicate-looking child of strumous diathesis, has had diarrhoea for two days. Ordered $1\frac{1}{2}$ grains of oxide of zinc every three hours.

August 20th.—Better.

CASE 9.—*August 14th.* Robert C——, aged 2 years, has been vomiting and purging for a few days. Ordered 2 grains of oxide of zinc every three hours.

August 21st.—Purging and vomiting improved.

August 24th.—Quite better.

CASE 10.—*August 14th.* Elizabeth D——, aged 1 year and 9 months, a spare, delicate child, teething, and has been

"feverish" for a few days, for which a draphoretic mixture had been prescribed; vomiting and purging came on three days ago. Ordered 2 grains of oxide of zinc every three hours.

August 16th.—Bowels much better but still sick, ordered mixture to be continued and a spoonful of milk or arrowroot before each dose.

August 21st.—Better.

CASE 11.—*August 14th.* John H——, aged 6 months, a healthy-looking baby, had a convulsion, and was then seized with purging; vomiting being absent. Ordered 1 grain of oxide of zinc every three hours.

August 19th.—Purging better and child not so irritable.

CASE 12.—*August 14th.* Alice R——, aged 2 years, a strumous child, has had an apthous condition of the mouth for a fortnight, but this is much better, was seized with vomiting and purging a few days ago. Ordered 2 grains of oxide of zinc every three hours.

August 20th.—Better, mouth quite well.

CASE 13.—*August 16th.* Ellen Elizabeth H——, aged 1 year, a fine healthy child, but has no teeth as yet, was seized with vomiting and purging three days ago, ordered 1 grain of oxide of zinc every three hours.

August 20th.—Purging better, still sick; nausea chiefly.

August 27th.—Well.

CASE 14.—*August 16th.* Stephen J——, aged 4 years, a fine-looking little fellow, commenced to be purged four days ago; no sickness. Ordered 2 grains of oxide of zinc every three hours.

August 18th.—Much improved.

August 25th.—Well.

CASE 15.—*August 14th.* Mary H——, aged 55 years was seized fourteen days ago with vomiting and purging, the vomiting has quite ceased, but she is still purged after taking her food in the mornings; she has very little pain. Ordered 5 grains of oxide of zinc three times a day.

August 19th.—Purging quite better, ordered a tonic.

CASE 16.—*August 16th.* Mary Isabella S——, aged 11 months, commenced with vomiting and purging a week ago when chalk mixture was prescribed. Ordered 1 grain of oxide

of zinc every three hours on the 19th as the chalk mixture had not checked the purging.

August 23rd.—Quite better.

These cases are a few out of many in which I have prescribed the remedy with beneficial results, and are sufficient in number to illustrate the value of the medicine.

In treating the cases attention was paid to the diet,—milk, arrowroot, &c. being chiefly ordered.

REMARKS ON CERTAIN RECENT PAPERS ON THE ACTION OF ALCOHOL

BY DR. ANSTIE.

It will be remembered, perhaps, that at the recent meeting of the British Medical Association, it had been intended that I should open a debate on "Alcohol in pyrexia." The occurrence of unforeseen events not only prevented my reading the paper which for some time back I had been preparing, but also arrested some final observations which I was carrying on leaving them incomplete. The delay has so far been useful, however, that it has enabled me somewhat to extend the original plan of my research, and to include fresh points of inquiry, which materially bear on the question of the medicinal functions of alcohol. This enlarged investigation is not yet quite finished, and I prefer not to enter, with any fulness, upon the details of my new facts till I can present them to the profession as a whole. In the meantime, however, three papers on the action of alcohol have been published, two of which by their intrinsic value, and the remaining one by its remarkable incorrectness of assertion, seem to demand a somewhat early notice from me. The present articles will therefore be devoted to a preliminary review of recent statements respecting the action of alcohol; more especially the papers of Professor Binz (read before the British Association, at Bradford), that of Dr. James Ross (published in the *British Medical Journal* of October 4th), and that of Dr. James Edmunds, read before the physiology section of the British Medical Association. Instead, however, of criticising these three papers *seriatim*,

I propose to consider, successively, the chief points connected with the action of alcohol with which all of them deal.

1. The first thing which claims attention is the question, whether alcohol is destroyed within the body? and on this point I am glad to observe that the high authority of Professor Binz unreservedly supports the statements made by Dr. Dupré and myself. The readers of this journal are aware that I have always denied the assertion, so freely made by the followers of Lallemand, to the effect that alcohol was largely expelled by the excretory organs in an unchanged condition. Successive series of experiments enabled me to prove, with increasing cogency, that the idea of the non-destruction of alcohol in the body must have arisen from some most serious experimental blunder; and as late as last year I succeeded, with the able assistance of Dr. Dupré, in carrying the matter a step further. At my suggestion that gentleman undertook a series of experiments which settled some very important questions. He found, as I had constantly found, that the quantity of unchanged alcohol eliminated must be exceedingly trifling; but he also demonstrated the unexpected and very important fact, that what had so far been reckoned as alcohol (as judged by its action on the chronic-acid test, and by its capability of being oxidized into acetic acid) was either not alcohol at all, or, at any rate, not derived from alcoholic ingesta, since it could be readily detected in the urine, not only of himself after a prolonged abstinence from all stimulants, but of a teetotaller.¹ Professor Binz recognizes the importance and validity of this fact; and he further points out that these portions of wine or of spirits which undoubtedly are eliminated—namely, the volatile ethers which we smell in the breath of persons who have been drinking wine, brandy, whisky, &c., are themselves also capable of affecting the chronic-acid test precisely in the same way as alcohol. I regret to observe that Dr. James Edmunds, on the other hand, affected to suppose that it was still probable, as far as the most recent scientific information had carried us, that as much as one-third of the alcohol taken into the body passed out of it without undergoing chemical change,

¹ This applies only to the matters found in the urine after a *non-narcotic* dose of alcohol. Narcotism is followed by a little real diminution.

and characterised the observations of Professor Binz (to the above-mentioned effect) as contrary to the testimony of the most recent authorities. It is to be regretted, I repeat, that Dr. Edmunds should adopt this tone, because, though he is not known to have personally made any original researches on the action of alcohol, he is an able writer, and is accepted by the teetotal party as a valuable scientific champion on their behalf; and under these circumstances it is a pity that he should waste time in so important a discussion, by neglecting to notice facts of the highest consequence, which have been long before the world. It is, of course, very natural that those who have repeatedly pledged themselves to the opinion that alcohol is treated by the body exactly like a poison, and eliminated wholesale without undergoing chemical change, should be reluctant to admit that the whole theory was completely wrong: but it is a significant fact that, although the original statements of Lallemand have been repeatedly controverted during the last fourteen years, the scientific representatives of the teetotal party have made no researches whatever to test them with, but have contented themselves with a repetition of the totally erroneous statements of the French experimenters. And although Dr. Edmunds does not now so completely ignore the work which has been done during recent years, he is either ignorant of, or at least does not mention, the most recent and the most important results of accurate scientific inquiry. It is obvious that Dr. Dupré's discovery of a constituent of the (unalcoholised) organism which behaves towards reagents in precisely the same manner as alcohol itself was a final and fatal blow to the theory of the elimination of considerable percentages of alcohol, and as such, Professor Binz at once recognized it; yet Dr. Edmunds thought proper still to talk of the probability that as much as one-third passes away out of the body unchanged.

I may conclude what I have to say on the subject of elimination by stating briefly that my own new researches on this subject (which will be shortly published) have enabled me to place this question beyond all possibility of further doubt or cavil. It had been objected, against the results obtained by Dr. Dupré and myself, that we had made no positively accurate estimation of the whole amount of alcohol which is eliminated by the

lungs and skin; and even Dr. Parkes (who candidly admitted the accuracy of our statements with regard to the urine) thought that we might have somewhat seriously underrated the amount of elimination by these channels. Obviously, the only way to meet this surmise was to employ a Pettenkofer's chamber, so that the whole excretions—including the breath and sweat—of the alcoholised animal might be collected, and the total eliminated alcohol exactly estimated. Without going into details, I may now say that this has been done with dogs;¹ and that the result conclusively proves that neither the lungs nor skin even of an animal wholly unused to alcohol eliminate any material quantity of alcohol which has been ingested. But the whole evidence will be shortly in the hands of the profession.

2. The next question is, whether alcohol can *actively* subserve the functions of life, and thus deserve the name of a food? The time is not ripe for a complete inquiry into the possibility of alcohol being utilised for the actual construction of certain varieties of tissue; and I am free to allow that the evidence, so far, on the whole supports the belief that, with certain exceptions, alcohol is active rather in the direction of repressing than of forwarding the growth of new structures. But the growth and development of tissues is but one of the purposes—and not the most important one—which are fulfilled by foods; the rapid production of available *energy* is an object of the highest consequence both in the healthy physiological state, and still more in certain morbid conditions; and it is now the prevailing belief among physiologists that food may be the source of bodily energy without ever having entered into the composition of tissue. No source of energy more likely to be available could be named, according to chemico-physical theory, than a hydrocarbonaceous body, like alcohol, which possesses strong tendencies to combine with oxygen, and which, on being taken into the stomach, rapidly diffuses itself throughout the system. Those who deny that alcohol can in this way be a source of energy within the body have hitherto mainly rested on two supposed facts—(a) that of the elimination of large quantities of alcohol in an un-

¹ Subbotin, whose research was otherwise open to numerous objections, made the fatal mistake of employing rabbits, the most unsuitable animals that he could have selected.

changed condition, and (b) that of the failure of various observers to detect any of the known oxides of alcohol in the bodies, or in the excretions of drinkers. The first of these assumptions is now proved to be absolutely the reverse of fact: as to the second, my most recent investigations will not only show that alcohol is destroyed within the body, but will suggest the probable nature of the change. But without discussing these latest facts (which will be best narrated in a continuous form) we may say that a highly oxidisable hydrocarbon which in large quantities disappears within the organism, *must* have undergone changes which would evolve a large quantity of energy, either vital or *visibly destructive*.

The more modern objection to the idea that alcohol is a source of energy available for bodily functions is found in the remarkable fact (first noticed by Lichtenfels and Fröhlich, and recently more fully illustrated by Dr. Ringer and Professor Binz), that so far from raising the temperature of the body, alcohol, in any considerable dose (even short of intoxication), somewhat lowers it. Those who deny the food-action of alcohol find, in this fact, an argument, plausible enough to the unthinking, against the idea that a substance which acts in this way can possibly be a source of energy available for true function. But again, we have a right to complain of the heedlessness of those who thus argue: for they ignore the fact that common hydrocarbonaceous foods, such as fat, also do not raise the bodily temperature, though their oxidation in the body most assuredly does form a source of available energy. This fact, which was long ago observed by Ringer, which I have repeatedly re-stated, and which was pointedly dwelt upon by Binz in his recent paper read at Bradford, completely destroys the argument under this head.

I do not propose, however, to carry the discussion any further, in regard to this part of the alcohol question, until it is possible to lay the whole of my fresh evidence before the profession. It is sufficient to say here, that while there is strong *à priori* probability that alcohol can afford available energy for bodily function, not a single argument has yet been put forward which at all tends to directly disprove its possession of this power.

3. I turn now to another mode of action of alcohol by which it probably produces a large portion of the beneficial effects

which it can exert in the treatment of disease. Dr. James Ross is not quite right in supposing that Professor Binz explains the *whole* therapeutic action on the same basis as that of quinine—*i.e.* as a “protoplasm-poison” capable of restraining the movements and the multiplication of corpuscles and cells; for, as we have above seen, Binz fully admits the oxidation of alcohol within the body. But there can be no question of the great importance of the action which Binz has been mainly instrumental in forcing upon the attention of the scientific world; and in this point of view it is both interesting and remarkable to observe that the idea of this mode of operation of alcohol had occurred independently to three persons. It was first suggested, many years since, by Professor Beale, and the idea was very fully stated by him. But for various reasons it failed to obtain acceptance; to myself, for example, and I believe to many others, it seemed that too much weight was attributed by Dr. Beale to an influence analogous to, though weaker than, the coagulating influence of strong alcohol upon albuminous liquids. Moreover, this was in the days before Cohnheim’s rediscovery of Waller’s “migration” of blood-corpuscles had directed general attention to the extreme importance of the amœboid movements of cellular bodies. Professor Binz appears to have worked from an entirely independent basis, incited on the one hand by the migration-phenomena of Cohnheim, and on the other by the cellular theory of the process of fermentation. Hence he naturally speaks of alcohol as standing in the same rank with quinine, with which latter substance, as is well known, he also has obtained remarkable results in checking the movements and the multiplication both of the white blood-corpuscles of the blood, and also of the low organisms developed in putrefactive processes. And now Dr. Ross, who makes no particular pretensions to experimental discovery, and was in ignorance both of the older theory of Beale and of all the details of Binz’s researches, at once seizes, with characteristic clearness of logical faculty, upon the important inferences which must necessarily flow from that view of alcohol which places it in analogy with quinine as a “protoplasm-poison.” It may be said, by the way, that that phrase, *protoplasm-poison*, is a misnomer, and is dis-

claimed both by Binz¹ and by Ross² as inapplicable to the *therapeutic* arrest of corpuscular migrations. It is true that either alcohol or quinine will *kill* the white-corpuscles of the blood if applied to them in overwhelming concentration; but in their ordinary manner of action there is no reason to suppose that either of these substances impairs the vitality of the corpuscles. The word is as misleading, and as unfortunate in its practical tendencies, as is the retention of the phrase "heart-poison" as descriptive of the characteristic *medicinal* action of digitalis, now that we know that action to be, in fact, a tonic stimulation, as far as the heart is concerned.

Dr. Ross dwells much on another aspect of the action of alcohol upon cell-life as to which he frankly confesses that he had been (unknown to himself) pretty much anticipated by Beale. Certainly, he was completely anticipated by the latter observer, in the statement that the rapid growth of young-cells was restrained by alcohol, which promoted the fixation of tissues into those permanent forms which (in Dr. Beale's peculiar phraseology) are called "dead" matter. But the chief therapeutic use of alcohol suggested by Beale is its interference (especially in pneumonia) with the fluidity of the blood and the disintegration of the blood corpuscles, and a lesser but important effect he judges to be the hardening of the vascular walls, whereby they are still further rendered less permeable by the blood constitutes. So far as its influence upon physiologically growing tissues was concerned, however, Beale could scarcely have admitted (what Ross has the great merit to perceive) that a reduction of the rate of cellular growth might amount to an elevation, rather than a depression, of the vital status of the organ or the organism. *Pus*, as Ross rightly observes, is the very type of disorderly, over-rapid, and unstable formation: it represents mere dynamic explosion, with scarcely a tendency to tension equilibrium.³ And what occurs in the profuse formation of pus is only an exaggerated likeness of what occurs in many

¹ See his answer to Geltowsky's paper on the action of quinine, in *Practitioner*, October 1872.

² In his second paper, *Brit. Med. Journal*, October 25, 1873.

³ This is, in slightly different words, the same idea as was independently expressed by myself in "Stimulants and Narcotics," p. 103.

instances, of what would be generally considered mere physiological growth. The delicate and too rapidly growing schoolboy's tissues are in an analogous condition; and where it possible to check such over-formation to an exactly sufficient degree, the result would unquestionably be a higher, and not a lower, vital condition. Dr. Ross's examples of gin-stunted jockeys, &c., are instances of this practice carried to a baleful extent; the more moderate use of alcohol for a similar purpose has been tested not only by Dr. Wilks, but by myself long prior to the appearance of Dr. Wilks's interesting lecture to which Dr. Ross refers; and there can be no doubt that it does so act, most beneficially, in properly selected cases, although alcohol should not form part of a fairly healthy child's diet if he be growing moderately.

4. It is evident from what has been said above, that there is a perfectly good theoretic basis for the use of alcohol (could we find the right form and dose) in the early stages of acute inflammations, with a view to prevent the migration, or the too rapid destruction, of blood cells. Whether in practice the effect which we desire can be really attained is a point that I do not think has been quite settled as yet, though it has been warmly debated. Here, again, I may hope that some of the new observations which I am about to publish shortly will assist to the formation of a more stable belief on this subject than at present exists.

5. On the great subject of pyrexial temperatures, and the power of alcohol to reduce them, we now possess a very large mass of information; and to no one are we more indebted for this than to Prof. Binz. The opinions which he has formed on this subject rest on an enormous number of observations made, in part by himself, and in part by his pupils. But it will be impossible to adequately consider what has recently been done by Binz and others in the present paper; and I must defer the consideration of this and other work which has been recently done in connection with the use of alcohol till our next number.

[Unusual pressure on our space compels us to defer the Reviews and Bibliography.—ED. PRACTITIONER.]

Clinic of the Month.

Replantation of Teeth.—Dr. Isidor Lyons observes that replantation of teeth as a useful remedy in dental surgery has never been received with much favour by the dental profession. The results of cases prove, however, that at least there are conditions in which it is of great value.

The art and science of dental surgery has not yet arrived at such a high state of perfection that we have a remedy for every disease to which teeth are liable, unless we consider extraction as one, a remedy too often used, unfortunately.

The principal affections for which extraction is practised are necrosis, acute inflammation of the pulp, acute and chronic periodontitis, exostosis, and caries. Of these necrosis and exostosis are the only diseases for which extraction is almost unavoidable; for the rest a tooth affected with any one of them may, in the great majority of cases, be saved by appropriate treatment. But in ordinary practice extraction is the usual remedy adopted. Unfortunately it is generally the wish of the patient, who, perhaps, having already suffered so much pain, prefers the speediest remedy. On referring to the various works on dental surgery, we find that remedies for different diseases of the teeth are so few in number that he who adds one remedy renders a service to humanity. Such an one is replantation of teeth for acute and chronic periodontitis. This was suggested by Mr. Coleman, after seeing the same remedy succeed for acute inflammation of the pulp of a lower molar tooth, which had resisted every other known kind of treatment.

The principal objection urged against replantation of teeth is, that if a tooth is extracted it must necessarily lose its vitality, and therefore the fangs undergo absorption, so that after a time it becomes useless, and must be extracted. Supposing the objection to be valid, as absorption is a long process, sometimes extending over years, it will have been a greater gain for a patient to retain his tooth for an indefinite period than to lose it entirely and at once; but it is no more necessary that a tooth after undergoing extraction and replantation should lose its

vitality than for a long bone to do so after fracture, with stripping back of the periosteum. The extraction of a tooth may be divided into two parts—firstly, the laceration of the alveolo-dental membrane periosteum; secondly, that of the nerve and bloodvessels supplying the tooth. As regards the former there is no reason why the alveolar-periosteum should not again unite to the tooth, seeing that if a piece of periosteum be stripped off a bone it will unite again if placed in contact with the bone and left at rest. The union of the divided ends of a nerve is also a recognized fact, but even supposing this latter impossible, the tooth would merely be in the condition of one which has had its pulp destroyed—a common operation in dental surgery.

The manner of performing the operation is as follows:—A tooth which is to be replanted should be carefully extracted, and as little as possible of the surrounding tissues lacerated: it should then, unless the operation be simply for the destruction of the dental pulp, and where the periosteum is healthy, be immersed in some antiseptic fluid, such as diluted carbolic acid or chloride of zinc (the latter from experience being preferred); the socket should then be swabbed out some half-dozen times with a strong solution of the same antiseptic employed. The tooth, if carious, should be plugged and returned to its place. If there is any thickening of periosteum, fibrous growth, sac of abscess, or absorption at extremity of fang, it should be excised before replantation. Should the patient complain of pain arising from the operation, prescribe poppy fomentations, although the pain is rarely more than what is due to the tenderness of parts from laceration of soft tissues after the extraction of the tooth.

Out of twelve cases that Mr. Coleman has operated on within the last four years, nine are successful and three have failed. The failures have but one significance, and that is, teeth to undergo replantation must be selected. In a cachectic patient the chances are against success; when a tooth has lost the support of its fellows on both sides, it cannot become firm. Nevertheless, the successful cases warrant a further trial of replantation, which would preserve many teeth otherwise sacrificed. The number of my cases during the last four years has been twenty-seven, some of which cannot be traced; the remainder are too recent to be judged. The reason why they are so few arises from the difficulty of inducing patients to believe in such a remedy as replantation. (*Lancet*, Sept. 6, 1873.)

The Preventive Treatment of Uric Acid Calculi.—Dr George Harley read a paper before the British Medical Association, limited to the means of arresting the formation of uric acid calculi, and facilitating the discharge of those not

already too large to be voided by the natural channel, which included all calculi not exceeding the size of field-beans. Tea, coffee, wines, and beers were to be prohibited, or, at least, prescribed in very great moderation, to patients labouring under the uric acid diathesis. He next alluded to the recent proposal of Dr. Day, of Victoria, to give ozonic ether in such cases, and passed on to the consideration of the alkaline treatment. From the very earliest times, alkalis had been resorted to with the view of retaining uric acid in solution until its expulsion from the body; and what the ancients did empirically we moderns did scientifically by improved methods, and with much greater success. The alkalis now in most general use were soda, potash, and lithia, in the form of carbonates, citrates, and acetates. Ammonia, on the other hand, was avoided in the uric acid diathesis, on account of the salt which it formed being less soluble than any of the others. The common idea was, that the action of alkalis in the uric acid diathesis was solely and purely a chemical one. There no doubt existed a chemical action, and that a most important one; but, beyond this, there was an important physiological action produced in the body, through which the oxidation process was so much increased as to transform the whole soluble uric acid into the very soluble urea. To Dr. Basham we owe the establishment of this as a clinical fact. In the treatment of the uric acid diathesis, more depended on the dose than on the kind of alkali given. As a general law, it was unnecessary to render the urine more than neutral, except in cases where we were attempting the dissolution of stones already formed; but, even then, there was danger in making the urine either too alkaline, or retaining it in an alkaline state for too great a length of time. Dr. Nunneley found that from ten to eighteen drachms of citrate of potash in twenty-four hours notably diminished the excretion of urea; and Dr. Basham found that half-drachm doses given three times a day augmented it to even double or treble its previous amount. Dr. W. Roberts, of Manchester, found that, while sixty grains of carbonate of potash to a pint of water daily dissolved twenty per cent. of an uric acid calculus, the solvent power of the solution gradually diminished as the solution was made weaker or stronger. Dr. Harley then called attention to the very great importance of the quantity and quality of the drinking water. Patients who had suffered from gravel or stone in one district, frequently got rid of it on removing to another; and this he had been able to trace to the difference in the quality of the water. Hard water, especially that from chalky districts, causes stone; soft water cured it. He consequently recommended the free use of distilled water, not only as a menstruum for the medicine, but also for cooking purposes. Moreover, as

the more pure water taken, *cæteris paribus*, the more effectual was the treatment, he gave his patients, when possible, from twenty to forty ounces of filtered rain or distilled water in the twenty-four hours; and, where they objected to its unpalatability, a squeeze of lemon or a pinch of salt was added to it. Hard water must, in all cases, be avoided. The only substantial benefit derived from mineral waters was, he believed, that the medicine was there given in a very dilute form. In mineral waters the relative proportions of their ingredients ought to be regulated according to the age, constitution, state of health, and other special requirements of the patient. As regarded the benefit of mineral waters in the uric acid diathesis, he pointed out that, contrary to some recent published opinions, it was due chiefly to the alkaline salts they contained. Dr. Harley concluded by saying that the chief obstacle to our success with chemical therapeutics in the treatment of calculi lay in the imperfect knowledge of physiology and chemistry possessed by practical men, who almost invariably failed in their endeavours to combine science with empiricism.

The use of the Electro-Magnetic Current in the second stage of Labour.—Dr. McRae, of Penicuik, records three cases which occurred to him in 1868 and 1869, in which the electro-magnetic current was employed to shorten the second stage of labour. In three cases the os was dilated and the uterus flaccid. The poles of a galvanic machine (strength of current not stated) were applied. His mode of application is as follows:—One of the electrodes, a flat piece of metal, the size of two hands opened, and joined in the line of their long axis and curved to fit the transverse abdominal curve, is applied to the abdominal parietes, and retained by the patient's hands, a layer of blanket being between the metal and the fingers to prevent the current entering by the fingers. The other pole is placed against the perineum, and retained by folds of blanket or a small pillow. The current causes contraction both of the uterus and of the abdominal muscles. When the head descends and comes towards the perineum he shifts the perineal electrode on to the coccygeal region, thus exercising a *vis a tergo* and relieving the perineal muscles of their tonic contractions, which now become flaccid and permit dilatation. The head is thus born in the proper line of the outlet with freedom. (*Edin. Med. Journ.*, Sept. 1873.)

Faradism in Post-partum Hæmorrhage.—Dr. R. C. Macintosh, of Doncaster, reports the following case, which may be considered of interest as furnishing an instance of the efficacy of a mode of treatment for the great opprobrium of obstetric art, which, if not new, is little known, and at any rate far from

being so widely employed as its numerous advantages would appear to indicate that it should be. The patient was in an extremely feeble state of health, and subject to epileptiform seizures. Convulsions came on during the first stage of labour, and could only be checked by keeping her under the influence of chloroform for some time. Failure of uterine action occurred before the os uteri was fully dilated; but as it was sufficiently dilatable, forceps (Beattie's) were introduced, and delivery was accomplished; still the uterus did not contract, and after the placenta was removed, hæmorrhage could only be restrained by keeping the hand within the uterus. Grasping and kneading the uterus, cold effusions externally, and injections of cold water *per vaginam*, produced no effect. A dilute solution of perchloride of iron was freely injected into the uterus, but proved ineffectual. The employment of faradism was then suggested as a *dernier resort*, and one of Stöhrer's portable coil machines was procured. An interrupted current of considerable intensity was directed through the uterus, one pole of the battery being placed upon the abdominal walls immediately over the fundus by means of a curved plate of copper, and the other applied to the cervix. Almost immediately firm contraction took place, and when the current was discontinued after a short time the uterus remained securely contracted, and no further hæmorrhage took place. The patient made a good recovery. Dr. Macintosh remarks that to those who have often witnessed, as has probably been the lot of most general practitioners, the bustle and confusion and other disagreeable accessories of a case of obstinate hæmorrhage after labour, any means that tend to diminish these inconveniences must prove acceptable. The use of galvanism as a means of treatment in these cases, besides its remarkable efficacy, as shown by the foregoing case, and which might naturally be expected, judging from its action on contractile tissues, generally possesses many other advantages over the means usually employed; for instance, the cleanliness and simplicity of its mode of application, and the avoidance of those evil consequences in the puerperal state which are so apt to result from the irritation of the uterus, and the exposing and deluging with cold water which usually take place. (*British Medical Journal*, August 9, 1873.)

Extracts from British and Foreign Journals.

The Use of Digitalis in Maniacal Excitement.—In a paper communicated to the *Journal of Mental Science*, Dr. Julius Mickle, of the Grove Hall Asylum, Bow, observes that though many writers have advocated the use of digitalis in various forms of insanity, such as acute paralytic or epileptic mania, there yet appears to be great diversity of opinion as to its efficacy, great variety in the methods of administering it, and a wide range in the quantities prescribed. While one has been content to order five to ten minims of the tincture three or four times a day, another has given, in similar cases, single doses of half an ounce of the same preparation. Having used a variety of medicines for the alleviation of restlessness, agitation, quarrelsome irritability, noisy and threatening language, or impulsive violence, as observed in certain forms of insanity, Dr. Mickle was so struck with the calmative effects produced by digitalis that he was induced to examine its action systematically, though without neglecting the use of other means. Tincture of digitalis (B. P.) was the preparation almost exclusively used, and of this the average dose was thirty minims three times a day. Amongst the cases in which it was employed were many of chronic mania in which the course of the disease was chequered by paroxysms or exacerbations of excitement. Forty-one patients in all were treated with the digitalis, and the length of time they were under treatment varied from seven days to nearly three months. Dr. Mickle gives the details of a number of these cases, from which it appears that the paroxysmal recurring excitement of chronic mania was *decisively* relieved by digitalis. In some instances, however, the effect was only moderate, and in others it was *nil*. In most cases of chronic paroxysmal mania the pulse rises considerably during the paroxysmal excitement. And digitalis, when it checks the paroxysms, is found to reduce the high pulse associated with them: the pulse, however, was not always or necessarily retarded. Dr. Mickle observes that with every desire to avoid hasty theorizing, it was difficult to resist the speculation that

in the cases benefited by digitalis, maniacal symptoms were associated with disorder of cerebral circulation (whether originating in local arterial spasm or relaxation), which modified the nutrition of the convolitional mass, and that the method of operation of digitalis was either by giving tone to the heart's action, so as to overcome the tendency to local vascular stasis and congestion, or by acting more directly on the peripheral arterioles — intracranial or other — and thus immediately influencing the cerebral circulation. In a few of the cases some gastric disturbance was produced, and in these the use of the medicine was suspended or the quantity given was diminished. In like manner any marked alteration of the cardiac rhythm or sounds was felt to indicate immediate cessation of its use. Cardiac contra indications were, however, extremely rare. (*The Journal of Mental Science*, No. lxxxvi.)

The Use of Mineral Waters in the Treatment of the Diseases of Women.—Dr. Macpherson remarks that the best way to discover what were regarded as the principal baths for women is to inquire what baths have been considered the great cures for sterility, for no waters can be considered to operate more favourably in restoring the uterine system to a healthy condition than such as promote fecundity. The first large bathing establishment famed in such cases in the days of the Romans was the warm sulphur bath of Sinnessa or Mondragone in Italy; in later times we had the Balneum Caiæ at Viterbo, Cithara, in the island of Ischia, and Bormio, in the Rhoetian Alps, which last place, we have sacerdotal authority for saying, "never defrauded any noble lady of her hope of becoming a mother." Crossing the Alps, the hot waters of Baden, in Switzerland, are found in great repute towards the close of the middle ages. So, also, in Germany, those of Gastein, which were the most renowned of all, of Liebenzell and Schlungenbad; and at an early period the value of the waters of Spa, Schwalbach and Pyrmont was discovered. In France, some of the best known waters were the warm ones, such as Bourbonne, Lancy, and Plombières or St. Sauveur, where ladies are alleged to leave their pessaries behind them as votive offerings. At a later period other waters grew into favour, such as the warm alkaline waters of Vichy, St. Nectaire and Ems, or waters, also alkaline, containing aperient salts, such as Franzensbad, Carlsbad, or Marienbad. And, still later, the use of salt waters, whether carbonated, as those of Kissengen, or the stronger brine waters of Kreuznach and its congeners. It is only of late years that attention has been paid in Europe to the effect of mountain climates. Besides the ancient baths of St. Moritz, Bormio, and Gastein, and the Pyrenean hills, many excellent baths for

women are at an altitude of from 1,200 to 2,000 feet, as, for instance, Franzensbad, Elster, Marienbad, Griesbach, Rippoldsan, Ischl, and Reichenhall. The lower Pyrenean baths, as Luchon and Bigorre, do not exceed 2,000 feet, but others range up to Caunterets, which is 3,250 feet. Dr. Macpherson then discusses the uses of mineral waters taken internally. The most popular are chalybeates, with a large supply of carbonic acid, as at the Spas of Schwalbach, Pyrmont, Spa, Bocklet, and St. Moritz. These are especially adapted for chlorotic and anæmic conditions. French and English chalybeates are deficient in carbonic acid, and less pleasant to take. If more action on the intestinal canal be required, if the constitution be scrofulous or lymphatic, some of the carbonated salt waters, not of a lowering class, such as Soden, Kissengen, or Homburg, are indicated. Where there is some congestion of the uterus or hypertrophy of it, purgative waters, such as Carlsbad, Marienbad, Franzensbad, or the iron waters of Rippoldsan, containing some aperient salts, are appropriate. Here also come the alkaline waters of various strengths, such as Vichy, St. Nectaire, and Ems; the last, perhaps, the most popular ladies' bath in Europe. Small quantities of brine containing iodides and bromides are drunk at Kreuznach, Krankenheil, and Castrocaro, in aid of the resolution of uterine tumours and hypertrophy. Dr. Macpherson next considers the general baths, and their effects, both alone and when supplemented by carbonic acid; peat baths, and baths impregnated with pine leaves. Finally he alludes to the chief modes of local irrigation and douches. (*The Obstetrical Journal of Great Britain and Ireland*, No. 4. July, 1873.)

Treatment of Rheumatic Peritonitis.—In a paper read before the Medical Society of the county of Albany, New York, Dr. I. M. Bigelow shows that rheumatic peritonitis is not so rare a disease as has sometimes been supposed: that it attacks those who are liable to rheumatism generally, that it is characterized by the tenderness, on pressure, of the belly and colic, the tympanitis, the sudden cessation of articular pain, and tendency to death by asthma by which it is accompanied. He gives the following suggestions for its treatment:—It should consist, he says, mainly in rest, opium, and permanganate of potassa, with a liberal use of nourishing beef-broths, milk, and alcoholic stimulants. When there has been time to prescribe for the rheumatic disorder prior to the peritoneal invasion, permanganate of potassa in $\frac{1}{2}$ grain doses should be given every hour; the bowels should be opened by an enema, and opium given freely, so as to relieve pain, subdue inflammation, and secure rest. Gelseminum also has been recommended, and, by some, is said

to be successful when given in the form of the fluid extract in three minim doses every hour. Quinine also may be employed, with a view to its tonic effects. Externally, leeches, rubifacients, turpentine, also spongio-piline, and hot water applications are well spoken of by authors. Besides these, Niemeyer says "that cold compresses to the abdomen exert a most beneficial effect;" and he agrees fully with those who consider the application of leeches to the abdomen, the use of cold compresses, and the internal administration of opium as the most effective treatment. Flint and Professor A. Clark each assert that our main reliance in treatment is opium given fully and freely. Cathartics, mercury, and blisters are alike condemned by most clinical observers and successful practitioners; although there are those who affirm that epispastics do frequently afford relief, and that mercury does arrest or check the inflammation. When an early dyspnoea and cyanosis admonish us of the imminence of fatal symptoms, venesection as a temporary expedient may, as Niemeyer observes, be resorted to, for we know of no other remedies to fulfil this urgent indication. To relieve vomiting, ice is best administered; to check the diarrhoea, opium is to be preferred to all other astringents: as the flux is due to cedema of the intestinal mucous membrane, opium is as beneficial as astringents. When convalescence is established, cod-liver oil and quinine should be liberally prescribed. If the exudation be great, iodide of potassium and acetate of potassa in small doses should, if borne, be ordered in connection with the various preparations of calisaya bark. Above all, fresh air, judicious exercise in the open air, nutritious diet, and quietude of body and mind, should be strictly enjoined. Cathartics should be given with great caution, and not for at least six weeks after recovery; enemata are to be preferred. The tendency to frequent relapses should always be borne in mind, and fatigue, exposure to cold, and inattention to diet strictly forbidden. (*Philadelphia Medical Times*, vol iii. No. 83, 1873.)

The Employment of Mint for the Suppression of the Milk.—Dr. Dasara observes that the knowledge of the antilactiferous properties of mint appears to have been possessed in very ancient times, since Dioscorides mentions the fact in his works, and subsequent writers have only confirmed his statement. Linnæus observed that cows that ate mint in their pastures yielded a very serous milk, and Laewis affirmed that the coagulation of milk in which some leaves of mint were placed was retarded. More recently, M. Desbois de Rochefort, experimenting on mint, found that fomentations of mint applied to the breast, and the infusion taken internally, were capable of suppressing the lacteal secretion, and of preventing the usual accidents attend-

ing milk fever in puerperal women. Trousseau expressed some doubt respecting this action of mint in his treatise on *Materia Medica*. But Dr. Pasquale Pepre, in a note on Trousseau's observation, remarks that the fresh leaves of mint placed in the axilla are commonly used in Naples to suppress the milk. Dr. Dasara determined to experiment for himself, and gives the details of a series of cases in which he tried the effects of the application of mint poultices made from the young sprigs at various periods of lactation, and the following are the conclusions at which he has arrived:—1. It is an established fact that mint has the power of suppressing the lacteal secretion. 2. The suppression of the secretion takes place at whatever period of lactation the mint is employed. 3. The effect takes place in a very short space of time, according to his experiments in from three to five days. 4. The suppressive action of mint can be localized to one breast. 5. No danger, nor even any inconvenience arises, either to the mother or child, either from the use of the mint or from the suppression of the secretion. Signor Dasara nowhere states in his paper the species of mint he employed; the omission is to be regretted. (*Rivista Teorico Practica. Fasc. vi., 1873. Giugno.*)

Treatment of Whooping-Cough with Quinine.—Dr. B. F. Dawson has recently read a paper before the Medical Library and Journal Association of New York, on "The treatment of Whooping-Cough with Quinine," of which the following is a summary:—Dr. Dawson referred, in opening, to this mode of treating pertussis, as first advocated by Professor Binz, of the University of Bonn, in 1870, through the columns of the May issue of the *American Journal of Obstetrics*. This author had shown that quinine had accomplished valuable results in his cases of the affection, which he considered to be a neurosis of the pneumogastric nerve, caused by infectious and irritating mucus that has accumulated in the pharynx and larynx; and having found by experiments that quinine destroyed, even when highly diluted, all structures found in normal mucus, he supposed that the mucus of pertussis also would be affected in a similar manner by quinine. In this he was not disappointed, the trial equalling his expectations.

The author mentioned the fungus theory of Dr. Letzerich, of Germany; also Dr. Breidenbach's mode of treatment—namely, the efficacy of the hydrochlorate of quinine, in doses of from $1\frac{1}{2}$ to $15\frac{1}{2}$ grains. He thought that with such strong testimony in favour of the quinine treatment of pertussis, it was somewhat surprising that nothing, or very little, had been done in this country to test its value.

Having opportunities for testing the value of anything new in

infantile therapeutics, this mode of treatment was applied in nineteen cases occurring in dispensary and private practice, and of these nine were selected as the most striking in their results. Out of the nineteen cases the shortest cure was effected in one day, and the longest—a single case—in twenty; the next longest being twelve days. In but two cases was he disappointed in the efficacy of the quinine. They were two dispensary cases, and the failure was attributed to the negligence of those in charge of them, the quinine not being given to them as frequently as ordered. There was, however, in both these cases some palliation of the paroxysms.

In regard to the administration of so disagreeable a remedy, he found that, though frequently there was some difficulty in getting the children to take it, yet it was exceptional for them to resist after the first two or three doses, and in only a very few did it cause vomiting. The direction to give the children a piece of an orange, or a little sugar five or six minutes after taking the quinine, had doubtless much to do with their seeming willingness to take the "bitter medicine." For his own part he accepts the fungus theory of Dr. Letzerich as the correct explanation of pertussis, and in consequence considers it an affection of the mucous membrane of the pharynx and larynx, and the "whooping" as simply reflex; and the fact that almost all remedies given for other than their local effects have either signally failed or but partially succeeded, he thinks strengthens this hypothesis.

Nevertheless, he does not attribute the rapid cure effected by quinine to the simple destruction of the fungus, but also to its nauseating, bitter taste. In every case of pertussis there is an abnormal secretion of a thick tenacious mucus from the mucous membrane of the pharynx (whether this secretion is due to simple catarrhal or reflex hyperæmia, or to fungoid development it matters not), which may or may not excite a paroxysm of whooping, but which certainly aggravates and prolongs the latter, as may be proved by the fact that the paroxysms invariably cease the moment this mucus is removed either by the coughing, vomiting, or the finger. The effect of a small amount of a solution of quinine, when taken into the mouth and swallowed, is instantly, from its bitter and nauseating taste, to excite a free secretion of thin mucus from the buccal mucous membrane and the salivary glands, and thus softening, render easy of dislodgment the tenacious mucus referred to. The frequent repetition of the quinine, therefore, keeps up this free secretion, and thus prevents the mucus from becoming tenacious and difficult of dislodgment. At each act of coughing the accumulated mucus is readily loosened and expectorated, and unobstructed inspiration obtained. The rapid loosening of the cough, the briefness of

the attacks in comparison with those previous to the administration of quinine, and the easy expectoration, tend to favour, he believes, the correctness of the above theory.

In conclusion, the speaker felt convinced that if the following rules are carefully observed, few, if any, will be disappointed in their results :—

1st. Give the quinine—sulphate or hydrochlorate—dissolved by acid in pure water only. For children under three years, from five to eight grains, and for older children and adults, from ten to twelve grains to the ounce.

2nd. Give not less than a teaspoonful *every hour*, or at the longest every two hours during the day, and whenever cough comes on in the night.

3rd. Give nothing afterwards for some minutes to destroy the taste or wash out the mouth.

4th. Continue giving it, notwithstanding the first doses may be vomited.

5th. Be sure that the quinine is pure and thoroughly dissolved.

In the foregoing paper the author wished to be understood as advocating the value of quinine in curing the “whooping” chiefly, the cough in some of the cases lasting for some time after the whooping ceased, and which required the usual treatment for bronchial catarrh.

Department of Public Health.

THE DISPOSAL OF SEWAGE.

THE Committee on the Treatment and Utilisation of Sewage, appointed by the British Association for the Advancement of Science, presented a summary report of "final opinions" (as the reports of proceedings in the daily journals phrase it) at the Bradford meeting. This report is of curious interest, for it deals not merely with the treatment and utilisation of sewage as mechanical or chemical economical questions, but it includes also the health-aspect of these questions. Under what circumstances the Committee held itself called upon to express its opinions upon the health-aspects of the questions submitted to it is not stated, and cannot be easily inferred. The constitution of the Committee, while admirable in view of an examination of the economical questions attaching to the treatment and utilisation of sewage, is not such as to inspire confidence in opinions which it might express on the health-aspect of these questions. Indeed, having regard to that constitution, it is difficult to conceive that the Association contemplated the Committee extending its labours beyond the subject of modes of treatment in relation to the purification and utilisation of sewage. The different reports of the Committee which have come under our observation do not show, moreover, that the Committee at any time has had before it the evidence which would have enabled it to deal with the health-aspect of sewage disposal, except, it may be said, incidentally, and to a very limited extent, in relation to parasitic disease. It is with some amazement, therefore, not unmingled with regret, that we find the Committee stepping beyond the limits of the duty imposed upon it, and

giving expression to a series of opinions upon so grave a matter as the health-aspect of sewage disposal. The blunder committed by so learned a body is as astonishing as the conclusions expressed on this particular subject are crude. Upon all that relates to the purification and utilisation of sewage we give heed to the opinions of the Committee with the greatest respect; but it is otherwise when the Committee undertakes to deal with questions for which its members, with one exception, have no special qualification, and regarding which it could not have the materials for a sound judgment before it.

We give the Report of the Committee in detail :—

"1. All conservancy plans, including midden, heap, and cesspool systems, dry ash and dry earth closets, pail closets, &c., are quite incompetent 'as solutions of the general question of the removal of the refuse matters of a population; they only deal with a small part of the liquid manure; towns which resort to one of them require to be sewered, and the sewage requires to be purified. The manure produced is in all cases (except in that of simple pails or tubs where no extraneous materials are added) poor, and, taking into consideration the cost of collection, will only bear the cost of carriage to a short distance—that produced by the dry earth system being, even after the earth has been used three times over, merely a good garden mould. Moreover, these plans all violate one of the most important of sanitary laws, which is, that all refuse matters which are liable to become injurious to health should be removed instantly, and then be dealt with afterwards. With all these plans it is an obvious advantage on the score of economy to keep the refuse about the premises as long as possible; and the use of deodorants of various sorts, or even of disinfectants, proves that this is the case, and] that these systems all depend upon a fallacious principle. They should therefore be discouraged as much as possible, and only resorted to as temporary expedients, or with small populations, in very exceptional instances.

"2. The water-carriage system, on the other hand, is based upon a sound principle, that of removing all the refuse matters at once and in the cheapest possible manner by gravitation, and ought to be resorted to in all but the most exceptional cases. The opinion of the committee that all sewers should be made of impervious materials, and that separate drains to dry the subsoil should be constructed where necessary, has already been most emphatically expressed. The freest possible ventilation of sewers, house-drains, and soil-pipes, in order to prevent accumulations of foul air, is also essential. With regard to the utilisation of sewage, the committee has come to the conclusion that the precipitation processes that it has examined are all incompetent, and necessarily so, to effect more than a separation of a small part of the valuable ingredients of sewage, and that only a partial purification is effected by them. Some of them may, however, be useful as methods of effecting a more rapid and complete separation of the sewage sludge.

"The upward filtration process only effects a clarification of the sewage, and is, therefore, no solution of the question.

"Weare's charcoal filtration process, as carried on at Stoke-upon-Trent Work-house, did not give satisfactory results, the effluent water being in effect weak sewage; an opportunity will, however, soon be given for an examination of this

process in a modified form on a much larger scale at Bradford, and under more favourable conditions. Intermittent downward filtration through soil has been shown at Merthyr Tydfil to afford a means of purifying the sewage under favourable conditions, but it cannot be said to be a method of utilisation except to a partial extent, as the investigations made by the committee showed that the effluent water contained as much nitrogen as was originally in solution in the sewage, but mainly as nitric acid, instead of as ammonia and organic nitrogen; and there can be no doubt that the process will prove useful as an adjunct to irrigation, or where a sufficient amount of land for irrigation cannot conveniently be got.

"By properly conducted sewage irrigation a solution is afforded to the question of sewage utilisation; it has already been stated that a precipitation process, or some clarifying process, may be found useful. In all instances it is essential that the land should be well underdrained, and that the sewage should all pass through the soil and not merely over it; otherwise, as has been shown, it will only occasionally be satisfactorily purified. The catch-water, or, as the committee has termed it, the 'super-saturation' principle, is not defensible either on agricultural, chemical, or sanitary principles; an irrigation farm should therefore carry out intermittent downward filtration on a large scale, so that the sewage may be always thoroughly purified, while at the same time the maximum of utilisation is obtained.

"It is certain that all kinds of crops may be grown with sewage, so that the farmer can grow such as he can best sell. Nevertheless, the staple crops must be cattle food, with occasional crops of corn; and it is also certain from the analysis of the soil that it has become very much richer, and that the manurial constituents of the sewage accumulate in it. Cattle should be fed on the farm, which leads to a vast increase in the production of meat and milk, the great desiderata of the population producing the sewage. Thus the system of farming must be specialised and capital concentrated, the absence of which conditions has proved a great barrier to the satisfactory practical solution of the sewage question.

"The committee has not been able to trace any ill effects to the health of the persons living around sewage farms, even when badly conducted; nor is there any proof whatever that vegetables grown thereon are in any way inferior to those grown with other manure. On the contrary, there is plenty of evidence that such vegetables are perfectly suited for the food of man and beast, and that the milk given by cows fed on sewage grass is perfectly wholesome. Thus Mr. Dyke, the medical officer of health of Merthyr Tydfil, states that since the abundant supply of milk from the cows fed on irrigated grass, the children's mortality has decreased from 48, 50, and 52 per cent. of the total deaths, to only 39 per cent.; and that so far from diarrhoea having been made more prevalent by the use of sewage cabbages, 'last year the Registrar-General called attention to the fact that diarrhoea was less prevalent in Merthyr than in any place in England and Wales;' and he expressed his belief in 'the perfect salubrity of the vegetable food so grown.'

"With regard to the assumption that has been made that entozoic disease would be propagated by irrigation, all the evidence that the country has been able to collect, and more especially the positive facts obtained by experiments, are against such an idea, and the committee is of opinion that such disease will not be more readily propagated by sewage irrigation than by the use of human refuse as manure in any other way, and probably less if the precaution be taken of not allowing the animals to graze, but always having the grass cut and carried them."

If we give regard, first, to that which is of chief value in this report, it is to be observed that there is no slight gain in having a standard method of disposing and utilising sewage to guide us. Now, the method of water-sewerage for the disposal, and of irrigation for the utilisation, of sewage, as advocated by the Committee, appears to us to afford such a standard as is needed. The opinions of the Committee in this respect concur with those of the leading authorities on sanitary matters, and they re-affirm the conclusions of the late General Board of Health. It is no slight confirmation of the soundness of these conclusions, that the prolonged and elaborate inquiries of this Committee and of the Rivers Pollution Commission in regard to the disposal and utilisation of sewage should have led, after an interval of near upon a quarter of a century, to the same result. Of the health-aspect of water-sewerage as compared with the other modes of disposal, we take it that no serious question can now arise; and of the health-aspects of sewage irrigation, when rightly conducted, there is no trustworthy evidence to show that it is harmful to people living upon or in the vicinity of the irrigated ground. We take objection, however, to the loose statement of the Committee, that "it has not been able to trace any ill effects to the health of the persons living around sewage farms, even when badly conducted." Surely, the events in connection with the sewage meadows at Eaton, in Northamptonshire, recorded by Dr. Buchanan last year¹ should have prevented a statement so unguarded as this. We take objection also to the loose reference to Mr. Dyke's looser implication and statement. However much it may be hoped that the sewage-farm at Merthyr may, by an abundant yield of milk from the cows fed from grass grown upon it, have contributed to the diminished children's mortality and lessened diarrhoea of Merthyr at the period referred to, a sufficient time had hardly elapsed from the period of establishment of the sewage-farm when Mr. Dyke's statement was made, to carry it out of the region of simple surmise. It would be interesting to know when and where the Registrar-General "called attention to the fact that diarrhoea was less prevalent in Merthyr than in *any place* in England."

¹ See the *Practitioner* for May 1873.

Water-sewerage, carried out with due regard to the principles so clearly set forth in Mr. Robert Rawlinson's well-known "Suggestions," published by the Local Government Board, and sewage irrigation conducted upon the principles advocated by the Committee, may be regarded as the standard mode of sewage disposal and utilisation. Wherever this mode can be compassed it should certainly be carried out. But when the Committee proceed to assume that water-sewerage may be universally adopted, and to express the opinion that any other mode of disposal should only be tolerated for temporary purposes and in very exceptional instances, they betray an unpardonable want of knowledge of the general bearings of the subject. The most cursory examination of the abounding and wealthy populations of Bradford and its vicinity would have shown them that to formulate such a conclusion was to formulate a proposition absurdly impracticable. •

If the Committee is prepared to show that a system of water-sewerage may be carried out *without water*, then there may be some justification for its conclusions. But until this be done the practicability of adopting such a system must be dependent upon the practicability of obtaining a sufficient quantity of water for the purpose. Bradford, to the present time, as also the neighbouring town of Leeds, have a mixed system of water-sewerage and midden closets. Both towns are expending enormous sums, which, in the fulness of time, may give them a sufficiency (not now enjoyed) of water, which, if the labours of the British Association Committee and others as to utilisation give confidence, may induce the municipalities to adopt water-sewerage generally. But the sources of water supply of both boroughs are not unlimited; and if the requirements of trade (Yorkshire people having a notion that it is desirable to live first, before dying) increase as they have been doing of late years, shrewd residents have a suspicion that the two boroughs may never be able to afford the supply required for water-sewerage, if the means of life are to be a first consideration. Moreover, immediately outside the limits of the two boroughs are great populations, whose only chance of a sufficient water-supply for trade purposes and domestic necessities is through the two healthier municipalities becoming their water-purveyors,

and it is in the highest degree improbable that the two municipalities could meet the whole of the requirements of the populations which might look to them, and whose readiest sources of large supply they have bought up. If this question of quantity of water-supply in relation to sewerage can make itself acutely felt in wealthy boroughs like Bradford and Leeds, what must be the chances of dealing with it when it occurs among less capable communities?

The Committee appears to have been unaware that the adoption of other methods of disposal of sewage than those of water-carriage and irrigation is, in a large number of cases, a matter of necessity and not of choice; and it has committed a like blunder to that which was committed by the late General Board of Health. That Board, like the Committee, saw clearly the enormous advantages of dealing with sewage by water-carriage and irrigation; and also, like the Committee, they discountenanced any other system of disposal of sewage. With more justification than the Committee, the Board failed to recognize the fact that water-sewerage could not be carried out without *water*, and that it must be supplanted by some other mode, or by other modes of disposal. The mischievous consequences of this error are only now beginning to be rightly understood. It long interposed an insuperable difficulty to sanitary improvement in the greater number of towns and villages, and it still presses upon sanitary workers. Local authorities were told in effect that they were not warranted in adopting any other mode of dealing with sewage than water-carriage, and that to this mode they must limit all their efforts at amendment in this respect. As a consequence, where water-sewerage was impracticable, matters in respect to human excrement were left untouched. The old filthy systems of disposal—the abominable cesspool and the horribly offensive middenstead—were retained; and probably in the greater number of instances the condition of middenstead towns is as bad now, and in some cases it is worse, than it was twenty-five years ago.

We have before us at the present moment an official report on Castleford, in Yorkshire, a populous village not distant from Bradford, which brings out the mischievous action of the Board of Health in this direction in a very clear light. Twenty-three

years ago Castleford was inspected by one of the engineers of the General Board of Health (Mr. Babbage). This gentleman, following the routine course of the Board, advised that Castleford, to get rid of its excremental abominations, should sewer itself and adopt water-closets. He suggested also a mode in which he believed water for the purpose might be obtained, the place being dependent for its water on surface wells. A preliminary matter was the formation of a Local Board, which was carried out, and the Board appointed a Medical Officer of Health—one of the earliest appointments of such an officer. Twenty-two years after the inspection of the General Board of Health, a medical inspection of Castleford was made by order of the Privy Council, in consequence of prevalence of fever in the village. This is what the inspector writes of "excrement disposal":—

"The common privy, with middenstead, is still the ordinary method of excrement disposal in Castleford. In 1850 it does not appear that there was a single water-closet in the village; at the present time the number of water-closets probably does not exceed nine, and it may be even less. Mr. Babbage's recommendation that water-closets be adopted throughout the village could not be carried out for the want of the necessary water-supply; and it is questionable whether the water-supply now about to be brought into use will suffice for their general adoption. The nuisance from privies with middensteads has not been lessened to any important extent since Mr. Babbage's inquiry. As a rule, they have been cleansed more frequently, and immoderate accumulation of refuse in them has probably been largely prevented by inspection. But the inherent evils of the old structures have remained unabated. Some of the middensteads are simply cavities excavated in the ground, without even such slight impediment to leakage of liquid filth into the subsoil as a brick lining would interpose. Others, and the greater number, are receptacles built of porous brick, uncovered, and generally of two or more (some of many) tons capacity. The oldest of these receptacles are inexpressibly foul, and of all it may be said, from the soakage into the walls and into the floor, that they are as productive of nuisances and as dangerous to health when empty as full."

In fact, old middensteads were twenty-two years ago foul from soakage and deterioration; and new middensteads were built upon the same plan as the old. With this state of things we are not surprised to learn that the total mortality from all causes in Castleford was not less in 1861—70 than it had been in 1840—49, and that the mortality from "fever" was greater in the latter than in the former period.

Now, it appears that when Mr. Babbage's recommendation as

to obtaining a water-supply proved to be impracticable, and that no water was to be had for water-sewerage, the Medical Officer of Health, Dr. Simpson, of Pontefract, suggested that measures should be taken to diminish nuisance from middensteads by covering them in, lessening their size, and cleansing them more frequently; and afterwards he suggested a pail-system (before such a system became familiar). But this recommendation, although it would have brought under control nuisance from middensteads, was not acted upon, because *it was opposed to the teachings of the General Board of Health*. This fact we have recently ascertained. The General Board had given no alternative; and the local authorities, when they could not act upon its suggestions, held it to be a duty or a convenience (it matters not which, the result being the same) to do nothing. In 1872 the inspector of the Privy Council advised the local authority at Castleford to act on the plan their Medical Officer had suggested five years before, as the only practicable means of diminishing the excremental nuisance in the place. Twenty-two years had been lost at Castleford by the General Board of Health not distinguishing between the practicable and the possible; and the same story may be told to a greater or less extent of almost every town and village in the West Riding of Yorkshire.

The Committee of the British Association has been blundering in like fashion, but happily not with the like capacity for mischief.

When the Committee talks of all modes of sewage-disposal, except water-carriage, "as quite incompetent as solutions of the general question of the removal of the refuse matter of a population," it talks nonsense. There is no general question in the sense meant. Each population must be dealt with upon its own particular requirements. Under the term "refuse matters" the Committee fails to distinguish those refuse matters which must be removed by scavenging and which cannot be removed by water-carriage. Hence an almost inextricable jumble. It is quite true that towns must be sewered, even if they do not pass their excrement into the sewers; but it is equally true that towns must be scavenged, and that the scavenger may be the only means available for the removal of excrements. It may be true that the different plans of sewage-disposal condemned by the

Committee "violate one of the most important sanitary laws, which is that all refuse matters which are liable to become injurious to health should be removed instantly, and then be dealt with afterwards;" but they all concur with an important rule of practice, that in dealing with an evil, if the best means are not available, we are not on that account to do nothing, but that it is incumbent upon us to use the next best means. In the last number of the *Practitioner* (October) we showed how different systems of excrement disposal, other than a water-closet system, may be carried out so as to be deprived of their most obnoxious features, and so to cause little nuisance.

The position taken by the Committee of the British Association is altogether an erroneous one. It is one thing to determine the best possible modes of sewage disposal and sewage utilisation; it is another and very different thing to ascertain the practicable application of such a mode under differing conditions of population. On the former question the opinions of the Committee are of value, and will command respect; on the latter question they are mischievously crude and tend to perpetuate the existing evil state of things in districts where water sewerage or sewage irrigation is impracticable. It is a fact that water sewerage, though the best mode of sewage disposal known, is inapplicable, as a sole system, in many towns and villages. It is a fact also that sewage irrigation, although it may be the best means of sewage utilisation known, is inapplicable in not a few localities. Other modes of sewage disposal are a necessity, and the lesson to be learned from this, in respect to the different other modes in use, is that in adopting them it is incumbent upon local authorities and upon individuals to carry them out in such a way as to avoid nuisance. This, as we have already shown, can very largely be done; and this is the lesson which it is so necessary to teach to the sanitary authorities and populations in localities where the middenstead or other dry system of excrement disposal is a necessity.

ON "DISINFECTANTS."

IN a paper in the German *Quarterly Journal of Public Health*, Prof. Alex. Müller, of Berlin, attempts to give precision to some of the current notions about infection and disinfection. He shows how the rough and ready practical application of these, and some other words, must be analysed and corrected before any scientific study of the nature of infection and disinfection can begin. Taking the term "infection," it is used in connection with some diseases, such as tinea or anthrax, of which the direct parasitic origin is unquestionable. To other diseases it is applied indifferently with the term "poison;" and in regard of these diseases, no endeavour is made to distinguish whether the poison has a direct operation of the nature of a chemical catalysis (as, for all we know to the contrary, may be the case in the action of the snake-bite poison), or whether the poison is generated as an indirect result of processes of growth and reproduction of organised particles, as, according to very common teaching, is the case with cholera. Herein it is observed that between the apparent alternatives other questions lie open, *e.g.* as to the nature of the changes known in the laboratory as catalytic; and as to the essential necessity of organic growth and reproduction to all or some of these changes themselves.

Classifying the material exciters of infectious disease as (1) the cellular contagious, (2) the molecular chemical or "poisonous;" and (3) the molecular mechanical, Dr. Müller points out that any scientific disinfection should begin with a knowledge about which of these has to be dealt with in the particular instance where infectious disease has to be repressed, as well as with a knowledge of the manner in which the infection operates—through the skin, through the digestive organs, or through the respiratory organs. Thence we might come to know whether mechanical removal will suffice, or whether chemical transformation is needed, or whether the destruction of vital property in the infectious material is the object to be sought.

No one supposes, of course, that such scientific accuracy is to be waited for before we do our best for warding off, in the way commonly understood by "disinfection," sources of danger to persons exposed to various communicable influences. The above principles of prevention have to be employed in practice with the best knowledge we possess, but it should be an object with those who have to give effect to any so-called "disinfection" to gather what facts they can as to the way of action, and as to the limits of usefulness of agents of various classes. Some infections, we know, will thrive better in acid than in alkaline media, some better in alkaline than in acid. Some appear very sensitive to cold, others to even moderate degrees of heat. All are capable of destruction by strong acids and alkalis, and all of them by burning. It is not to be forgotten that some agents may have an effect on the matter of infection in more than one way: fresh air, for example, effecting mechanical removal, desiccation, and oxidization at the same time.

In England we are in at least equal confusion with the Germans as to what are to be classed as "disinfectants;" for, to say nothing of Dr. Müller's mechanical agents, nor of deodorisers acting essentially on the inorganic accompaniments of infection, we must certainly recognise, as comprehended in our use of the term, agents acting in three quite different ways:—

1. Those which hasten ordinary decomposition, or substitute some more rapid form of decomposition; and thus, it is believed, get over quickly, or altogether avoid, the particular stage of decay in which special infection can grow and multiply.

2. Those which retard ordinary decomposition; and thus, it is supposed, hinder common organic matters from arriving at the stage of decay in which special infection can grow and multiply.

3. Those which, without ranking prominently in either of the former categories, have power of preventing the specific operation of infection, when opportunity for its operation comes to be given.

Of the first of these classes, permanganate of potash; of the second, zinc-salts; and of the third, sulphur, may be taken as

types. They are, however, types only; for we cannot, for example, in the present state of our knowledge, say that sulphur will destroy the specific property of every infection in the way it destroys the infection of itch, and perhaps of other diseases.

If disinfection were no practical question, but the mere destruction of whatever could be injurious were being aimed at, agents of the first class, and those of them which soonest reduce into inorganic combinations the organic elements both of the infection itself and of all associated matter, would undoubtedly give the truest means of avoiding injury. Fire, of all things, would be our resource in such case; and in fact the destruction of infected things by fire should, when this is a possible alternative, ever be held foremost in view. But the more potent agents of this class are obviously inapplicable to many things (to say nothing of persons) that require to be disinfected. And coming to the less potent agents, the efficiency of their action is seen at once to be, to a degree and in a sense that is true of this more than of any other class of "disinfectants," a question of quantity. With enough of earth, exercising its wonderful power of bringing animal matters into a state ready for vegetation, the whole of a mass of infected sewage may no doubt in time be made harmless, down to its most recalcitrant atom, germ, or ovum. With enough of fresh oxygen the whole of an infected bulk of air may in time be, not diluted only, but purified. But the practical question, "What is enough?" is for ever cropping up, and along with it the question of the order of destructibility of the substances to which these weaker destructive agents have to be applied. Chemists could probably tell us how many grains of permanganate of potash would be required for the complete reduction of a grain of albumen into inorganic combinations, and the information would be worth having. But they probably could not tell us the order in which various complex organic substances would be acted on by permanganate; where in the series would stand the material of small-pox; where that of enteric fever; and what, consequently, would be the quantity of the substance that would be enough to procure the destruction of these dangerous materials when mixed with a given quantity of other known substances. Even if chemists could tell us this, the words "given" and "known" in the proposition

point to assumptions utterly out of all question in any practical application. It appears necessary therefore to infer that agents of the present class can only be affirmed to be efficient as disinfectants when they are known to have reduced to an inorganic state the elements of all organic matters to which they are applied.

In this view of what can alone, with our present knowledge, be regarded as "enough" of a destructive disinfectant, what, we should like to ask, is the value of the inspector's sprinkle of chloride of lime on the top of a full and fever-infected privy-hole?

Of "disinfectants" of the second class, it must be admitted that, so far as they act as antiseptics, and as antiseptics only, there is no assurance of their value in any but one sense, but that an important one. To whatever extent the belief is true (and we know it to be true of particular infections) that the growth and multiplication of infection is promoted by the decay of associated organic matters, it must follow that the increase of infection is prevented in proportion as such decay of common organic matters is retarded; and thus is obtained an extension of the opportunity for mechanical removal both of the associated organic matters and of the infection, while limited in amount. The mere statement of the case, however, shows that an effect of incomplete and temporary value only is obtainable from these agents, so far as they are mere antiseptics. The infection remains, and when the retarding action of the antiseptic is over, then, *ex hypothesi*, other organic matters pass into their usual decay, and provide the infection with the means of further development. Briefly, the antiseptic class (as such) of disinfectants have to be regarded as essentially ancillary to other disinfection either of the chemically destructive sort or of the specific-infection destroying sort. It is scarcely necessary to say what enormous practical advantages may be gained from the antiseptic class of agents, *e.g.* from iron-salts in privy pits, from carbolic acid in sewers; but it is plain that too much is not to be expected from them. The quantity in which agents of this class have to be used is determined by different considerations from those which apply to agents of the destructive class. Our readers will not have forgotten the very important experimental results quoted in our review, last month, of Surgeon-Major O'Nial's paper in the recent Army Blue Book.

We must defer until next month consideration of the third class of disinfectants, of the little that is known of them, and of the line of work that must be taken for knowing more.

INDUSTRIAL DISEASES.

I.—THE POTTERY INDUSTRY IN ITS RELATION TO THE DEATH-RATE FROM PULMONARY AFFECTIONS.

IN an appendix to his report upon the sanitary condition of the Staffordshire Potteries, of which an abstract was given in the September number of the *Practitioner*, Dr. Ballard discusses the pottery industry in its relation to the death-rate from pulmonary affections, viz., phthisis and other diseases of the lungs. Tables are given showing the mortality per 100,000 of population living, from these diseases, separately and conjointly, at all ages and at different periods of working age from 15 years to 65 years, on an average of ten years, 1861-70, as respects the two registration districts of Wolstanton and Stoke-upon-Trent, and also as respects each registration sub-district.

The towns of Longton, Fenton, Stoke-upon-Trent, and Hanley, in the registration district of Stoke, and the towns of Burslem and Tunstall in that of Wolstanton, are the head-quarters of the Staffordshire pottery manufacture. It is estimated that in the Stoke registration district 312·5 per 1,000, and in the Wolstanton district 268·8 per 1,000 of the male population over 20 years of age, are employed in this trade; of the female population over 20, it is estimated that in Stoke district 177 per 1000, and in Wolstanton district 109·5 per 1,000, are similarly employed.

In a paper printed in the Fourth Annual Report of the Medical Officer of the Privy Council, 1862, Dr. Greenhow pointed out how the death-rate from pulmonary affections in the Potteries was connected with the unwholesome character of the manufacture, and the mode in which it was carried on. In the present Report, a comparison is instituted between the

pulmonary death-rate in the Potteries during the decennials 1851-60 and 1861-70, and also between the same death-rates in the Potteries during 1861-70 and similar death-rates in England and Wales during 1851-60, the new census tables for England and Wales not being yet available for comparison. Dr. Ballard also compares the pulmonary death-rates of each sub-district with the same standard, much to the disadvantage of the town of Loughton. The general result of his observations and comparisons is as follows :—

“ 1. That, during the years 1861-70, the pulmonary death-rate among persons (males and females together) between the ages of 15 and 25 in the pottery districts, has been less than the standard rate from pulmonary disease of England generally.

[The standard rate is based on the deaths of a different ten years, but it may safely be assumed to have undergone no important change.]

“ 2. That during the same ten years the pulmonary death-rate among persons above the age of 25 in the pottery districts has been higher than the standard in England generally ; and that this excess, comparatively small in persons aged 25 to 45, is very considerable in persons (especially in males) between the ages of 45 and 55, and still more considerable (in males reaching to the double of the standard rate) in persons aged between 55 and 65.

“ 3. That this excessive mortality from pulmonary diseases in persons over 25 years of age in the pottery districts as compared with England at large, is, generally speaking, on the increase. Between the ten years ending 1860, and the ten years ending 1870, it has increased in males aged 25 to 45, in males aged 45 to 55, in males aged 55 to 65 ; and also in females aged 55 to 65 ; but it has decreased in females below 55 years old.

“ All these facts are in accordance with the known influence of the pottery trade in causing lung disease. The diseases of the lungs set up by dusty occupations, among which the pottery manufacture stands prominent, are of slow progress, and exhibit themselves upon death registers only after they have rendered existence miserable during a number of years. (Fourth Report of Medical Officer of Privy Council, p. 19). Again, the increase of the pulmonary death-rate in recent years, as compared with earlier times among males of all ages after 25, will probably find its explanation in the circumstance of the male population of the district being employed in larger proportion now than formerly in the pottery trade ; while the decrease of such death-rate in females under 55 appears to be connected with the smaller proportion now than formerly of females so employed. The reason why females above 55 years of age are dying of lung disease in recent years more than before is probably that, in this class, many people are included who had been brought in increasing numbers into the trade before the recent movement against the employment of women, and that the women of this class are only now dying of the chronic pulmonary diseases from which they have long suffered during the years of their pottery work.”

Since the Report of Dr. Greenhow, improvements have been slowly making in the conditions under which the manufacture

of earthenware, china, and pottery generally is carried on; but only in this sense—that new establishments have been erected with various modern improvements, while the older establishments remain much as they were. The improvements referred to are—the use of steam power in place of hand labour in certain processes; the abandonment of the “pressure stoves,” for a mechanical contrivance by which the new-made articles are brought, by the mere turning of a sort of screen or partition, at once into a hot drying chamber; the construction of buildings in such a way that consecutive processes are carried on in adjoining parts of the premises, thus doing away in part with the necessity of carrying heavy weights long distances; the substitution of the “pug-mill” for the laborious process of “wedging” the clay; and lastly, the substitution of pressure frames for the slip kiln in getting rid of superfluous water from the mixture of clays. It is remarked that were it rendered compulsory to introduce these improvements into the older establishments, the buildings would have to be partially pulled down and re-constructed.

With respect to the conditions which Dr. Greenhow particularly alludes to in his Report as affecting the health of the labouring population engaged in the pottery manufacture, Dr. Ballard remarks:—

“1. As to the inhalation of dust. Probably some of the workpeople in the newer establishments are less exposed to this source of danger than they were formerly; but in the older establishments there is no change for the better in this respect. The little improvement that has taken place is chiefly in the direction of greater cleanliness of the workshops, and in the partial avoidance of the necessity of boys carrying materials, &c., backwards and forwards, and thus raising dust. In other respects I find no improvement at all upon Dr. Greenhow’s description. The china scourers, and the women who dust the sand from the baked ware especially, are as much exposed to danger from their occupation as ever they were. In one new establishment, however, I saw an arrangement for drawing off the silicious dust.

“2. As to lead poisoning, nothing has been devised to avoid the necessity of the dippers immersing their hands in the glaze. ‘Ground layers,’ who dust a preparation of lead upon some coloured wares, are still unprotected. In one establishment only did I see an arrangement for drawing off the dust from the bench at which this work was carried on.

“3. The heat of the workshops is moderated in the instances in which the flat-pressers’ stoves are done away with, and there is less exposure to vicissitudes of temperature where pressure-frames have been substituted for slip-kilns.

“4. The workshops where the printing of paper designs is carried on, and

where printing, painting, gilding, and burnishing of the wares are performed, are still in many instances overcrowded, and insufficiently ventilated, giving rise to oppressiveness and heat of the atmosphere of the rooms. Still something, it is said, has been done to lessen this evil.

"5. Generally, the ventilation of the workshops, except in the newest buildings is still bad. The workpeople object to fresh air; and even where special means of ventilation have been provided, they do their best to render them nugatory. And it is to be observed, that anything like a current of air though the old badly-constructed workshops, tends to dislodge dust accumulated on ledges, &c., and to increase in this way the dirtiness of the atmosphere. But ventilation need not involve draught; and with proper arrangements for the admission of air from without, and the removal of the workshop air, much of the dust might be quietly carried away. Further, along with the ventilation, the most efficient means ought to be adopted for preventing dust getting into the general atmosphere of the shop. Some of the more important of such means are the provision of an even flooring, the proper provision of a ceiling to shops having other work-rooms above them, the removal of shelves and ledges above the heads of the workpeople, and regular and thorough cleaning at appropriate times.

"To sum up, it must be said, that although some improvements have been effected, and are in slow progress in the pottery establishments, they have not yet been long enough in operation, or become so general, as to produce any marked reduction in the mortality from lung diseases among potters. Some time yet must elapse before this can fairly be looked for. The workpeople engaged in the new establishments, have all their lives previously worked under the unfavourable conditions of the older workshops. Even now, boys and girls, on commencing their trade, mostly enter at first these old establishments, only a comparatively small number of them being employed at the new works. The only advantages they possess now are those furnished by the wholesome provisions of the Factory Acts."

II.—PHYSICAL CONDITION OF CHILDREN EMPLOYED IN TEXTILE MANUFACTURES.

IN the course of the past year Dr. J. H. Bridges and Mr. Timothy Holmes were commissioned, by the Local Government Board, to report upon the health of women, children, and young persons engaged in textile manufactures, with special reference to the hours and ages of employment. Their report, published as a Parliamentary Paper, describes the different processes of the cotton, flax, jute, woollen, worsted, silk, and lace manufactures, gives the opinion of local medical practitioners upon the influence exercised by these manufactures on the health of women, children, and young persons employed in them, states the result of a medical examination of numerous children so employed, made under the direction of the Commissioners; and finally, gives certain statistical inquiries bearing upon the

subject of the report. The portion of the report which is of chief interest, as arising from the direct inquiries of the Commissioners, is that which refers to the examination of children. This examination was directed to the height, weight, measurement of the chest, dentition, rickets, scrofula, skin diseases, and the existence of flat foot; and with regard to the method to be pursued in examination, the medical inspectors who assisted the Commissioners were instructed as follows;—

“The children were to be taken in alphabetical order, or in their order as they stood in the schools, so as to exclude all possibility of selection. The children were to be measured with their feet bare. The measurements round the chest were to be taken round the nipple, after a full expiration, and after a full inspiration. The boys were to be stripped to the waist; in the case of girls the measurement was to be taken outside the dress (*sic*). *Dentition*.—Slight imperfections, or decay of the milk teeth, were not to be considered worth noticing. *Rickets, &c.*—Under this head were noted such appearances of the bones of the head, wrist, legs, and chest walls as indicated old rickets. Any deformities, except flat foot, were also to be noted here. *Scrofula*.—This was to be judged of by the presence of enlarged glands, scars in the neck, enlargements of joint ends of bones, when exposed, or marks of present or past strumous ophthalmia. *Skin Diseases*.—The exact disease was of course to be specified. *Flat Foot*.—This required no special explanation.”

The districts visited for the purpose of examination were divided into (a) urban factory districts; (b) suburban factory districts; and (c) manufactory districts; and, in all, about 10,000 children were examined. The general impression made by the factory children upon the inspectors was in many respects not unfavourable. As compared with the children of the east of London, or of the poorer parts of Liverpool, they were markedly superior. They did not appear to be more liable than ordinary country children to rickets or scrofula. The general health in most other respects seemed fairly good. Attention was, however, drawn from the first to the frequency of diseased dentition, and of the flattening or suppression of the arch of the foot.

The facts registered from examination are given in a series of

tables, and the results of a comparison are thus stated:—The children of *non-factory* districts are superior to the *factory children of factory parents* in the following cases:—

“*Height*: Males, at all ages from 8 to 12. Females, at all ages from 8 to 12. *Weight*: Males, at ages, 9, 10, 11, 12. Females, at ages, 8, 9, 10, 11, 12. *In measurement round empty chest*: Males, at no age. Females, at no age. *In measurement round full chest*: Males, at ages, 9, 10, 11, 12. Females, at no age. *In expansibility of chest, as shown by difference after expiration and inspiration*: Males, at ages, 8, 9, 10, 11, 12. Females, at ages, 9, 10, 11, 12.

“The difference in height steadily increases, in males from a superiority of $\frac{1}{4}$ of an inch at eight, to one of an inch and a half at 12; in females, from a superiority of a quarter of an inch at eight, to one of an inch and a half at 12. The difference in weight increases in males from an inferiority of half a pound at eight, to a superiority of 4.25 pounds at 12. In the case of females, from a superiority of two pounds at eight, to one of 4.5 pounds at 12. The children of non-factory districts are slightly inferior in the measurement round the empty chest; but this inferiority is more than compensated in the case of males, and is nearly compensated in the case of females, by the greater expansibility of the chest.”

Comparing *factory children of factory parents* with *non-factory children of non-factory parents*, a less striking difference is observed. In the larger number of cases, however, the advantage rests with the latter. Thus non-factory children of non-factory parents are superior to factory children of factory parents:—

“*In height*: Males at all ages. Females at all ages. *Weight*: Males, at ages, 9, 10, 11, 12. Females, at ages, 8, 10, 11, 12. *Measurement round empty chest*: Males, at ages, 9, 10, 11. Females, at ages, 9, 10, 11. *Measurement round full chest*: Males, at ages, 9, 10, 11. Females, at ages, 8, 9, 10, 11, 12. *Expansibility of chest*: Males, at ages, 11, 12. Females, at ages, 8, 9, 10, 11, 12.”

A comparison of *urban factory children* and of *suburban factory children* (in both cases irrespective of parentage) results in showing marked superiority of the suburban over the urban

factory children. But it is remarked that while the urban children are markedly inferior to the children of non-factory districts, not even the factory children rise to the level of children of non-factory districts (urban and rural).

With regard to the existence of diseased dentition, rickets, scrofula (*a*), skin diseases, and flat foot, we quote the tabulated results :—

(*a.*) Of 1,570 children in non-factory districts,¹ of all ages from 8 to 12, we find affected with—

Diseased dentition	56 or 35·6 per 1,000
Rickets	25 „ 15·9 „
Scrofula	28 „ 17·8 „
Skin disease	17 „ 10·7 „
Flat foot	27 „ 17·1 „

(*b.*) Of 2,569 factory children in large towns, of all ages from 8 to 12, we find affected with—

Diseased dentition	234 or 91·0 per 1,000
Rickets	31 „ 12·0 „
Scrofula	58 „ 22·5 „
Skin disease	55 „ 21·1 „
Flat foot	203 „ 79·0 „

(*c.*) Of 2,383 children in suburban and semi-rural districts, of all ages from 8 to 12, we find affected with—

Diseased dentition	138 or 58 per 1,000
Rickets	55 „ 23 „
Scrofula	64 „ 26·3 „
Skin disease	47 „ 19·7 „
Flat foot	100 „ 41·9 „

(*d.*) Of 818 children in large factory towns, not employed in factories, we find affected with—

Diseased dentition	71 or 87·3 per 1,000
Rickets	25 „ 30·7 „
Scrofula	12 „ 14·7 „
Skin disease	14 „ 17·2 „
Flat foot	25 „ 30·7 „

¹ York, Chester, Lancaster, Thirsk, Northallerton, Malton, Kendal, Keswick, Penrith, Sedbergh, Chapel-le-Frith, Chinley.

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ON THE INFLUENCE OF POSTURE ON "PRE-SYSTOLIC" CARDIAC MURMURS.

BY W. R. GOWERS, M.D.

THE influence of posture in altering or removing cardiac murmurs is a subject of such practical importance as to merit greater emphasis than it commonly receives in the current symptomatology of diseases of the heart. In particular, the frequency with which the so-called "pre-systolic" mitral murmur exhibits this influence is hardly yet recognised. Several cases have come under my notice in which, after a careful and skilled examination in the erect posture, a heart has been declared free from murmur, when, had the patient been made to lie down, a bruit would have been heard which could not have been overlooked. I believe that in most instances the murmur which precedes the first sound¹ and is commonly regarded as characteristic of mitral obstruction, is both louder and longer in the recumbent than in the erect posture, and that

¹ The common interpretation of the murmur, as preceding the second, but synchronous with the first part of the cardiac systole, is not very accurately conveyed by the term "pre-systolic;" and the truth of that interpretation being still doubted by many authorities, it would be an advantage if some other designation could be found—such, for instance, as "pre-tonal"—which might remove a statement of fact from the domain of controversy.

in many instances, more frequently than in the case of any other cardiac murmur due to an organic cause, it may be heard in one posture and not in another, may be loud when the patient is lying down, and inaudible when he is standing up.

The following cases illustrate this peculiarity. The first is also interesting as presenting a curious, and, as far as I am aware, unique instance of the reversal of the usual effect of posture upon the frequency of the pulse.

CASE I.—Charlotte J., aged 17, single; suffering from slight chorea of about two months' duration, chiefly affecting the left side. She had had several previous attacks, each lasting several months, in which the right side was the more affected. She had never suffered from acute or chronic rheumatism. In the recumbent posture the physical signs of the heart were as follows:—Dulness commenced, above, between the second and third ribs, and to the right reached, but did not extend beyond, the right edge of the sternum. The impulse had a well-marked heaving character, and was accompanied by a distinct thrill. Apex beat at fourth interspace in nipple line; felt also, but less strongly, in fifth interspace. Sounds at the aortic cartilage clear and without murmur. First sound roughened at pulmonary cartilage; second strongly accentuated, but not reduplicate. At base, first sound rough, but no actual murmur. Midway between base and nipple a murmur became audible, preceding the first sound. Just within the nipple it was somewhat rough in character, tolerably loud, and occupied the second half of the pause, seeming to be cut short by the occurrence of the first sound. Midway between the nipple and the ensiform cartilage it had similar characters. At the apex beat the murmur was slightly less rough, but a little longer, commencing earlier, but still separated by a distinct interval from the preceding second sound. On passing outwards it became fainter and was lost about the mid-axillary line.

In the erect posture, the apex beat remained in the same situation; the heart sounds at the base and at the aorta and pulmonary cartilages presented the same characters, but the murmur could not be heard. Within and above the nipple the first sound had a slightly booming character, and at the apex beat was a little rough, but unaccompanied by bruits

On gradually changing the posture from the vertical to the horizontal, it was found that the murmur became audible when the patient's body was inclined at an angle of about 45° with the horizon; and on returning slowly to the erect posture it ceased at the same inclination. The patient was examined repeatedly during several weeks, and, whenever tested, the cardiac signs and the effect of posture on the murmur were the same.

The effect on the pulse of change of posture was not always the same. Occasionally the frequency was unchanged, being 84 or 86 in both the recumbent and the erect, the temporary acceleration caused by the change of posture quickly passing. Much more often, however, the pulse was more frequent when the patient was lying down than when she was standing, the difference commonly amounting to about eight beats per minute. It was never more frequent in the erect than in the recumbent posture (except for a few seconds after the exertion of rising). The following is an instance of the condition usually observed:—On one occasion, after lying perfectly still for some minutes with the head raised on the sloping end of the couch, the frequency of the pulse was 100 per minute. The patient then stood up. For the first ten seconds the heart was beating at the rate of 120 per minute; the pulse then fell to 86, and continued at 84 or 86 as long as the erect posture was maintained—about five or six minutes. The murmur, distinct in the recumbent posture, ceased the instant the patient stood up, even while the heart was beating at the rate of 120 per minute. While reclining on the couch so that the trunk formed an angle of a little more than 45° with the horizon, the pulse was 82, and no murmur could be heard. On the patient gently sliding a little lower on the couch, so as to incline the trunk a little more towards the horizontal, the pulse rose to 90, and the murmur became and continued distinct. After she had been lying still for about five minutes, the frequency of the pulse was 100. She then raised herself a little higher on the couch, preserving exactly the same relative position of the head; the pulse at once fell to 84, and the murmur disappeared. Occasionally, towards the close of the patient's stay in the hospital, the fall on assuming the

erect posture was not permanent. For instance, after lying for a quarter of an hour, the pulse was 96, and the murmur loud. She then stood up: the murmur ceased, and the pulse, after a few seconds' acceleration, fell to 84, continuing at that rate for two minutes, then gradually rose to 96, and remained at that rate during the succeeding four minutes, the murmur being inaudible. She then lay down, and the pulse continued at 96, the murmur returning. On rising the pulse again fell to a little over 80 for three or four minutes, and then again became more frequent. On no occasion, when a uniform posture was maintained, was any other irregularity in the pulse observed.

CASE II.—William P., aged 16, suffering from hemiplegia. No distinct history of rheumatic fever could be obtained, but six months previously, and a few weeks before the onset of the hemiplegia, he suffered for about a fortnight from severe rheumatic pains in all the limbs, not specially in the joints. The cardiac signs whilst lying were as follows:—Cardiac dullness commenced, above, at the lower border of second rib, and extended to right, not further than the middle of the sternum. Impulse, slightly undulating, was visible in the second, third, and fourth interspaces, and could be felt in the fifth. Pulsation was also visible at the supra-sternal notch. Apex beat fourth interspace, nipple line. The sounds at the base and at the aortic and pulmonary cartilages were free from murmur or reduplication. At the apex beat a tolerably loud "churning" murmur occupied the second half of the pause, and ended sharply with the first sound. It was not heard an inch outside the apex beat, nor an inch immediately above or below it, but it was heard about an inch to the inner side of the apex, and further inwards became lost. Over the same area the first half of the pause was occupied by an extremely soft murmur commencing immediately after the second sound and running on into the louder murmur. Frequency of pulse, 84 to 88.

When the patient stood up both these murmurs disappeared. In the region in which they were audible the sounds were a little louder than in the recumbent posture; the first sound had a slightly rough character, and the second a faint reduplication, but no murmur could be heard. Frequency of pulse, 92.

Walking quickly about the ward increased the frequency of the heart's action and the loudness of the sounds, but produced no murmur. When the patient lay down, the murmurs returned at once. On changing the posture gradually, it was found that the soft diastolic murmur ceased, and the pre-systolic murmur became less loud as the erect posture was approached, but did not entirely disappear until the patient was standing. Even when sitting upright on a chair it could be faintly heard; when he stood upright it ceased at once. While sitting with body bent forward and legs horizontal on a couch (a constrained posture), it was loud, though less loud than when lying down. On another occasion the pulse continued at 84 in both positions. As soon as the transient acceleration caused by standing up had passed, the frequency of the pulse continued the same as in the recumbent posture.

CASE III.—George L., aged 45, suffering from incomplete right hemiplegia, the onset of which had been gradual, occupying about twenty-four hours, and unattended by loss of consciousness. He had had attacks of rheumatic fever at 25 and 40. In the recumbent posture, cardiac dulness was not increased. The impulse was diffused, moderately heaving. Apex beat in normal situation, but felt over too large an area. Impulse at apex attended with a distinct thrill. At the aortic cartilage was a systolic murmur, also heard, but less loudly, at the pulmonary cartilage. It was distinct at the base, but, on passing towards the apex, ceased at a line between the left nipple and the ensiform cartilage. The second sound was prolonged, at the aortic cartilage, into a short murmur, while at the base it was reduplicate. At the apex beat was a loud rough murmur, commencing very soon after the second sound and terminating at the first. This was well heard outside the nipple, but was lost in the axilla. It was also lost about an inch inside the nipple. The reduplication of the second sound at the base was lost in passing towards the apex, but at one point the reduplicate second sound and pre-systolic murmur were audible together, and the commencement of the latter was distinctly separated from the second part of the reduplication by an interval considerably greater than that which separated the two parts of the second sound. Audible only in the

immediate neighbourhood of the apex beat was a systolic murmur very high pitched, almost sibilant in character. When the patient stood up, the apex pre-systolic murmur became inaudible, the other murmurs retaining the same or nearly the same characters.

CASE IV.—Female, aged 25, suffering from subacute rheumatism. Physical signs in recumbent posture:—Cardiac dullness reached, above, to the third rib, and to the right as far as the middle of the sternum. The apex impinged in the fifth interspace, an inch and a half within the nipple. Impulse slightly heaving, accompanied by a distinct thrill. At the base the sounds were free from murmur, the first being somewhat rough. At the aortic cartilage a well-marked systolic murmur was audible. At the pulmonary cartilage the sounds were clear. At the mitral apex a rough murmur preceded the first sound and ended with it. The second sound was succeeded by a short pause, and then the murmur was heard, which occupied about the latter two-thirds of the “silent interval.” It was heard in the axilla, but was loudest between the nipple and ensiform cartilage, where it had a well-marked “churning” character. When the patient stood up, the thrill was still felt; the first sound at the apex was clear, without any bruit, nor could the murmur heard in the recumbent posture be detected anywhere. The aortic systolic murmur was unchanged. The pulse was 112 in the recumbent, 116 in the erect posture, but the patient was nervous. When she was examined on a subsequent occasion, the murmur could be heard distinctly when the patient was standing, although it was much less loud than when she lay down.

It is difficult to suggest any explanation of the occasional peculiarity in the pre-systolic murmur which these cases illustrate. Their details, however, would seem to exclude several possible interpretations. They show, for instance, that the change in the murmur is not concomitant with any special alteration in the frequency of the cardiac contractions, since it was observed alike in cases in which the change of posture rendered the pulse more frequent or less frequent, or left it unaltered. They show also that the peculiarity cannot be ascribed to any simple effect of gravitation on the movement

of the blood through the mitral orifice. It might be suggested, for instance, that during the pause in the heart's action a larger proportion of the blood in the auricle flows into the ventricle in the erect than in the recumbent posture, and that thus less is left for the contracting auricle to force through the narrowed orifice. But in Case II. a diastolic murmur, distinguishable from that which immediately preceded the first sound, was heard in the recumbent and ceased in the erect posture, in which, had such influence of gravitation been effective, it should have been intensified.

I have had no opportunity of ascertaining *post mortem* the exact condition of the orifice in any of the cases in which this peculiarity was observed. It is conceivable that the varied mode of support of the heart in the recumbent and erect posture may affect the shape of the mitral orifice. Thus, when the patient lies, and the heart comes to rest, as it must do, on the organs behind or beside it, the auriculo-ventricular opening may be so changed, in the passive state of the ventricle, as to lessen its already diminished area; and in other cases such change of form may bring a roughened surface into further contact with the blood-current.

The effect of the change of posture on the pulse in the first of the above cases is of considerable interest. It illustrates the influence of the nervous system in effecting the adjustment of pulse to position, and it suggests some objections to certain theories which have been proposed as to the mode in which that influence is exerted.

NOTES OF FIVE CASES OF PARACENTESIS THORACIS.

BY SYDNEY RINGER, M.D.

I AM induced to publish these few cases because they assist to answer some important questions concerning pleurisy with effusion and the operation of paracentesis thoracis. They show, what indeed is well known, how slight a disturbance this operation causes, and what immense relief it affords. They show that the operation may be usefully employed in the febrile and non-febrile period of the disease, and that during fever the fluid may be withdrawn by the aspirator, and not accumulate again. They prove, moreover, that in some cases of febrile and non-febrile empyema it is sufficient to withdraw part of the fluid by the aspirator, and that the rest of the pus may disappear; and that it is not always necessary to lay open the chest in order that the pus may drain entirely away. Moreover, they show that in severe empyema the temperature may be normal, or scarcely at all raised, and in those cases accompanied by chronic fever the pus may be perfectly sweet.

The following case of simple acute pleurisy of the left side occurred in a child twenty-five months old. On the tenth day of the disease, and when the fever was still high, the fluid had accumulated in such quantity that the heart beat a little inside the right nipple, and percussion dulness reached to the right edge of the sternum. The breathing was greatly oppressed, and the face and hands were livid. Indeed, it was obvious that tapping afforded the only hope of saving life. Mr. Durham, in the presence of Sir William Jenner, Mr. Clover, and myself, tapped the chest over the cardiac region, and withdrew with the

aspirator twenty-three ounces of clear serum. The child was at once relieved; the breathing became calm, the lividity disappeared; indeed, the improvement was so great that the friends asked if all danger was passed. However, as we expected, the fluid rapidly re-accumulated, so that in twenty-four hours the symptoms and physical signs were as bad as before the operation. Mr. Durham again tapped the chest (twelfth day of the disease), and withdrew with the aspirator seventeen ounces of rather turbid fluid, probably slightly purulent. The child was again greatly relieved, though less than after the first operation. He was, however, so exhausted that he died early next morning. The first operation caused scarcely any pain, but towards the end of the second the child became somewhat distressed.

The foregoing is a good example of one common form of acute pleurisy leading rapidly to effusion, and where the fever lasts usually only five to ten days. The following successful case belongs to another and common type of the acute disease with serous effusion, where the disease often sets in insidiously, and the fever lasts about twenty-eight to thirty days. It shows that even during the fever period the fluid may be withdrawn by the aspirator without any return of the effusion; nay, that the fluid left in the pleura may entirely disappear before the fever declines, and that in cases of this kind the operation may not affect the fever. Of course in these cases the patient is greatly relieved, and no doubt, if the operation is performed early, the lung is saved from great permanent disablement.

Anne Bannister was admitted into University College Hospital under my care on the 8th Sept., 1873. It was difficult to learn exactly the time the illness began. A month before admission she complained of pain in the right side and shortness of breath; in a fortnight a severe hacking cough set in. When admitted there was some lividity of the face, with fulness of the jugular veins on lying down. A physical examination showed that the right side contained a very considerable quantity of fluid. There was general bulging of the right side, with obliteration of the intercostal spaces. Percussion dulness reached across the sternum to a finger's breadth to the left of the sternum. There was very slight resonance in the right

interscapular region, and close to the spine in the infrascapular region. As is often the case, notwithstanding this extensive effusion, slight vocal fremitus was felt over the right back. Over the greater part of the diseased side, except at the base, both back and front, the breathing was very amphoric, and most perfect pectoriloquy was heard. The heart beat about two inches outside the left nipple. On Sept. 10, Mr. Crocker, the resident assistant in charge, made an exploratory puncture with a hypodermic syringe in the fifth interspace in the posterior part of the axillary region, and withdrew a small quantity of serum. The trocar of the aspirator was then inserted at the same spot, and fifty-six ounces of clear serum was withdrawn. As is usual, the withdrawal of the fluid and the expansion of the lung excited some cough, but less than generally occurs. The serum, when put aside, in a short time set into a solid jelly. A small portion, when boiled, coagulated and became solid. The position of the heart was not much altered by the operation, and though the resonance improved, much dulness remained, there being good resonance in front as low as the fourth rib, but the rest of the chest remained almost absolutely dull. Day by day the right side slowly retracted. On the 27th the retraction, both front and back, was considerable. There remained considerable relative dulness of the whole of the right side; this, as usual, being most marked in the axillary regions. The patient was sent to Eastbourne, and on Nov. 8 the physical signs were almost the same as before she left; thus the right shoulder was much depressed, the right side much retracted, especially behind. There was relative dulness over the whole right front, the dulness increasing as we descended the chest, and becoming absolute below the nipple. Respiration was very weak, blowing, and towards the base rather cavernous; the expiration was much prolonged. Expansion was very deficient. The percussion note was natural, behind, in the supra-spinous fossa and interscapular region; slight resonance below, close to the spine, but absolute dulness external to this and in the axillary regions. The respiration was very weak. The heart-sounds were very distinct over the right side, much more so than over the left. A slight cough remained, with slight shortness of breath on exertion. She was in excellent

health. In order to learn if the remaining dulness were due to serum, condensed lung, or lymph and thickened pleura, several exploratory punctures were made with a hypodermic syringe soon after the operation, but no fluid was withdrawn, except on the first occasion; hence we concluded that the serum left was absorbed. On the day of admission her temperature rose to $103^{\circ}6$, and continued to rise daily to 103° and $104^{\circ}5$ till the 16th (operation Sept. 10). On the evening of the operation it rose $0^{\circ}4$ higher than on previous or subsequent days. On the 16th the temperature only reached 102° . It then gradually fell daily. On the 17th the maximum was $101^{\circ}6$; on 18th, $100^{\circ}4$; 19th, 100° ; 20th, 101° ; 21st, $100^{\circ}2$; 22nd, 101° ; 23rd, $100^{\circ}6$; 24th, $100^{\circ}6$; 25th, $99^{\circ}6$; and from this date, with the exception of an occasional slight elevation, it remained natural. The temperature was taken in the rectum six times daily, at 3 A.M., 7 A.M., 11 A.M., 3 P.M., 7 P.M., and 11 P.M.

The next two patients suffered from right-sided empyema. In one case there was no fever; in the other the temperature rose to $99^{\circ}4$, and occasionally to 100° Fahr. But $99^{\circ}4$ is scarcely beyond the limits of health in a child. These cases show that there may be no fever in very extensive purulent pleurisy; and as both patients completely recovered after tapping with the aspirator, they prove, contrary to the opinion generally held, that it is not necessary in cases of empyema without fever to lay open the chest, nor to use a drainage-tube to permit the pus to drain completely away; but that the pus left after tapping with the aspirator may become rapidly absorbed. The pus in both these cases was quite sweet.

Arthur Shaw, aged two years, was admitted under my care Aug. 2, 1873. Three months before admission he began to lose flesh, his appetite became bad, and he passed blood and slime with his motions. He grew gradually worse, till seven weeks ago he was too ill to leave his bed for three weeks. At this time he first began to cough. He was taken to Brighton without benefit, his cough growing worse. On returning to town his mother took him to Mr. Langmore, who discovered pleurisy with extensive effusion, and sent him into the hospital. On admission we found the whole right side almost absolutely

dull, except in the interscapular region, and the dulness reached across to the left edge of the sternum. His heart was a little displaced, the maximum impulse being in the nipple line. On Aug. 4, Mr. Crocker, at my request, tapped the chest in the fourth interspace, just outside the right nipple, with the aspirator trocar, inserting it about two inches, but as only blood escaped it was withdrawn, and an exploratory puncture made with a hypodermic syringe in the fifth interspace in the axillary line, and some pus was withdrawn. The aspirator trocar was then inserted at the same spot, and fifteen ounces of sweet pus withdrawn without a bad symptom, the front of the chest becoming much more resonant. The breathing was much relieved. On Aug. 6 we noticed that the right side began to retract. As a good deal of dulness over the greater part of the right side remained, especially of the axillary region, we determined to tap a second time, and Mr. Crocker thrust the aspirator trocar through the sixth interspace (I think in the axillary region, but unfortunately on this point no note was taken), and withdrew six ounces and a half of sweet laudable pus. The operation excited no bad symptoms. From the first operation (and the second in all probability was unnecessary) the child was much more comfortable, and he greatly improved in appetite and strength. His side continued to retract, and on Sept. 3 the right side was decidedly smaller than the left. There was an excellent percussion note over the front of the right chest, in fact the note was hyper-resonant. There was great dulness of the whole back, and still more so of the right axillary regions.

On Sept. 5 the child was attacked with sore throat, which raised the temperature for a few days. On Sept. 24 the physical signs were much the same, but the dulness was a little less marked over the back, and, as usual in these cases, it was most marked in the right axillary regions, growing less on passing backwards to the spine, except at the extreme base, which continued equally dull to the spine. The spine was straight, and the heart in its natural position. In order to learn if the remaining dulness was due to fluid, several exploratory punctures were made at different times with a hypodermic syringe, without obtaining any fluid. These exploratory punctures

caused very little pain, and no subsequent trouble. The child was sent into the country, and returned in perfect health, the physical signs remaining the same.

On the day of his admission (Aug. 2) his temperature remained normal. Next day it rose to 100° , and on Aug. 4, the day of operation, it rose to $100^{\circ}4$; and subsequently rose daily to 99° , sometimes to $99^{\circ}6$, and in a few rare instances to 100° . As I have said, for a few days there was decided fever from sore throat.

Ellen Jane Moore, aged $4\frac{1}{2}$, was admitted Sept. 1, 1873. In May she was seized with "inflammation of the lungs." She recovered slowly, but on Aug. 24 was again ill, and "had a return of her old complaint." On admission the child was rather emaciated, and exhibited a strongly-marked tubercular look. Her breathing was rather laboured. There were evidences of extensive effusion into the right pleura. The right side was enlarged and absolutely dull, with the exception of slight resonance in the interscapular region and close to the spine below. The dulness reached to a finger's breadth to the left of the sternum, and the respiration, as is often the case with children, was amphoric over the upper half of the right front and sternum. The heart's impulse could be seen and felt over a very extended area, reaching from the epigastrium to two fingers' and a half breadth outside the left nipple. On Sept. 4 we determined to perform paracentesis. An exploratory puncture was made in the fifth interspace in the axillary line with a hypodermic syringe, and some purulent fluid was withdrawn. The aspirator trocar was then inserted at the same spot by Mr. Crocker, and nineteen ounces of greenish yellow, sweet pus withdrawn. The operation caused very little pain, and no bad symptoms. After the operation the heart beat one inch outside the left nipple. All the child's chest troubles at once left her, and from this time she steadily improved, gaining flesh daily. A good deal of dulness remained after the operation. The chest slowly retracted. The breathing continued cavernous over the right front for some time. On Sept. 27 there was great dulness below the nipple, and much relative dulness over the back, the most resonance being situated in the interscapular region and close to the spine

below ; the dulness, as usual, increasing as we passed outwards, and becoming excessive in the axillary regions and also at the extreme base, even close to the spine. The movements of the right side were much impaired. The heart beat in the nipple line. To ascertain if the remaining dulness was due to pus or serum, several exploratory punctures with a hypodermic syringe were made over the dull part of the chest, but without obtaining any fluid, and the child was sent to Eastbourne for a month, whence she returned in the most robust health.

The maximum temperature before the operation was 99°. On the evening of the operation and next day the temperature reached 99°·4 ; for the next eight days it only rose to 99°. Subsequently, on one or two occasions it rose to 100° Fahr. Like the former case, the child had an attack of fever, beginning on Sept. 12, and lasting four days, the temperature rising daily to 100° and 101° Fahr.

The temperature in these two cases was taken in the rectum six times daily, at 3 A.M., 7 A.M., 11 A.M., 3 P.M., 7 P.M., and 11 P.M.

The last patient, William Ringe, aged 47, was admitted under my care, Aug. 2, 1873, with right-sided empyema. There was daily fever, the temperature rising to 101°·5 and 102°·5, and sometimes to 103°. In addition, however, to the empyema, he expectorated pretty freely ; and two days before paracentesis he spat a slight streak or two of blood. During the operation and for a few days after, the streaks of blood were much more numerous, indeed the sputa were deeply stained. This ceased, and he did not spit blood again. The expectoration, with this small quantity of blood, led us to think there might be phthisis as the cause of the empyema, but there were never any physical signs to lead to such a diagnosis, and moreover the expectoration grew much less, and the temperature became natural, and the patient wonderfully improved in health—in fact, looked and felt quite well. We felt bound, therefore, to conclude that probably there was no phthisis. If this opinion is correct, this case shows that febrile empyema of some standing may recover after tapping without laying open the chest or draining off the pus with a drainage tube. If the patient were phthisical, it shows that, with this grave complication, mere tapping with the aspirator may be sufficient to cure.

In this case the disease began acutely on July 15 ; previous to this he was in perfect health, with the exception of occasional attacks of gout. On that day he was seized with vomiting, back-ache and headache, and became light-headed. On the third day cough and diarrhoea set in, the diarrhoea continuing ever since, occurring directly after food. He had pain on the right side and at the pit of the stomach. The illness had considerably reduced his strength.

On admission, the chest movements on the right side were much impaired in front, except under the clavicle. Percussion note under the right clavicle was high pitched, and there was almost absolute dulness in the third interspace, and quite in the fourth. Vocal fremitus could be felt over the first and second interspaces, but not below. The left side was healthy ; percussion note was rather hyper-resonant, and rhonchal fremitus could be felt over the whole left front and over the right apex. The heart's apex could not be felt. The right back scarcely moved at all, and was a little less prominent than left. Vocal fremitus could not be felt on the right side below the angle of the scapula. Percussion high pitched over upper part of right back, and absolutely dull below the angle of the scapula. Some sonorous rhonchus was heard at the left base. On a subsequent occasion a little mucous rhonchus was heard over the right back. The breath sounds were very weak over the lower half of the right side. On the 17th a puncture was made with the hypodermic syringe in the eighth interspace of the right side, in a line with the angle of the scapula, and a few minims of pus were withdrawn. Next day (18th) paracentesis was performed at the same spot with the large aspirator by Mr. Barlow, the resident officer in charge of the wards, and thirty-four ounces of pus were withdrawn, of the consistence of arrow-root, with numerous curdy flakes. The pus was quite sweet. The canula had to be cleared out several times, and once or twice withdrawn and cleared and reintroduced ; and once it was inserted through the seventh interspace. The patient suffered a good deal of pain in the muscles of the side on moving ; this, however, soon wore off, and he felt decidedly relieved by the operation, although the physical signs were scarcely altered, there being only a slight improvement in the percussion note of the right back. The vocal

fremitus was unaltered. Mucous rhonchus was heard in a few days over the whole of the right back.

On the 24th the signs were very little altered, the fever and expectoration continuing as before, but he expectorated with greater ease. On the 29th we noticed that there was increased retraction of the whole of right side, and the shoulder had sunk a little. There was scarcely any movement of the right side. Full vocal fremitus could be felt over the right base behind, and there was no absolute dulness below the angle of the scapula. The rhonchus had greatly decreased. The right side continued to retract. Vocal fremitus was distinctly felt over right base and axillary regions. Coarse rhonchus was heard over the left lung. His fever remained high for some time, and then declined and became natural; his general condition at the same time wonderfully improving, and he was sent to Eastbourne quite convalescent. An exploratory puncture was made just before he left, over the dull part of his chest, with a hypodermic syringe, but no fluid could be detected in his chest.

This last patient appears to be a good instance of a not uncommon case, where there is a history of slow loss of flesh and strength for weeks. On examination dulness is detected at one base, with diminished or natural or even increased vocal fremitus. The lower part of the side is rather retracted, and respiration is very weak. There is considerable fever every night, rising to 101°, 102°, or 103° Fahr. The symptoms continue perhaps for several months, the retraction of the side slowly increasing, and then the fever gradually falls, at last becoming natural, when the patient improves in strength and gains in flesh. In these cases it is extremely difficult to know if the symptoms are solely due to empyema or are due to phthisis also (there may be a good deal of expectoration without phthisis); and unfortunately the temperature does not help us to decide.

A CASE OF CONTRACTED CICATRIX AFTER A BURN OF THE NECK, TREATED BY PLASTIC OPERA- TIONS.

BY MR. W. SPENCER WATSON, F.R.C.S.

Surgeon to the Great Northern Hospital, &c.

ALFRED FOWLER, aged 7 years, was burnt severely on the face, in front of the neck, and under the right arm, a week before Christmas 1871. The result has been, that the cicatrix has contracted in the neck to such an extent as to evert the lower lip completely, and to tie the chin down to the neck, the line of the profile of the neck being straight in a vertical direction from the chin to the sternum, and the skin of this region being tense and thrown into vertical folds.

The boy's aspect was extremely frightful—the mouth remaining constantly open, the saliva dribbling constantly from the angles of the mouth, and the head being held forwards and inclined somewhat as if to relieve the tension of the neck.

There were scars all over the face, and both cornea had suffered by the burn. Ulcers had formed on them, and on healing had left leucomata. The skin of the face, however, above the mouth had not contracted, so that no serious deformity such as ectropium had arisen at this part of the boy's face.

During the healing of the ulcer, I am told by Dr. W. H. Day, of Islington, under whose care the little fellow then was, the dressing employed consisted of a paste of oil, prepared chalk, and carbolic acid, and I have no doubt that the excellent result, so far as the face was concerned, is due in great measure to the judicious treatment then adopted. No doubt the depth to which the burn had extended in the neck prevented a similar good result being obtained there.

In Sept. 1872 he was brought to me at the Great Northern
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Hospital, with a view to attempting some means of alleviating his condition. And as ten months had elapsed, and contraction seemed to have reached its limit, I determined on Sept. 18 to operate.

The following operation was performed, while the patient was under chloroform.

(1.) A transverse incision was made in the sound skin of the neck, just below the cicatrix. The skin and subcutaneous tissue were then dissected upwards, so as to free the chin completely and allow the lip to be raised nearly to its normal position.

The effect of this first step in the operation was to leave a large triangular raw surface in the upper part of the neck, the lower angle of which reached half-way down to the sternum.

(2). In order to fill up this gap, two triangular flaps were taken from the sides of the neck, the skin there being sound; and (3), these two flaps were crossed one over the other, in such a way that their prominent angles fitted into the retreating angles left by the first incision, and were retained in their new position by sutures.

The lines of incision were then painted over with styptic colloid, and covered with a thin layer of cotton-wool, and the part lying beneath the skin was supported by a pad and strap carried up to each side of the face and temples. A thick layer of cotton-wool was then laid over the whole, and this kept in position by means of a light bandage.

Four days after the operation a discharge was found to be collecting under the crust of styptic colloid, at several places, and it was therefore removed and replaced by carbolised oil on lint. A small slough formed under the chin, and another at the thin point of one of the flaps, but the deep surfaces of all the flaps united by the first intention.

On Sept. 9—three weeks after the operation—the flaps were found to be firmly adherent in their new position, and the lines of incision also united, except at one or two small surfaces where granulations were springing up and healthy cicatrization was promising.

On Oct. 30 all had healed, and the lip was in very good position; but it was found that the mucous membrane was still everted, and on looking at it closely it was found that a pouch

had formed under the centre of the prolabium, binding it down to the upper part of the chin.

An attempt was therefore made to free this invaginated piece of skin and to replace the lip, by making two lateral incisions from within the pouch outwards to each side, and then stitching together the raw edges.

This only partially succeeded, as the stitches broke away very soon, and the surfaces were too tender to allow of the necessary pressure to keep them in position during cicatrisation.

At the same time a horizontal slip of the mucous membrane of the mouth inside the lip was removed and the edges stitched together.

The effect of this combined operation was not very great at the time, but subsequently, in February 1873, the improvement in the boy's appearance was considerable, and he could then keep his mouth shut, and was scarcely at all troubled by the escape of saliva. Since the last operation he had a chin strap of elastic webbing, which was fastened on the vertex of the head.

This had tended to keep the parts in good position during the consolidation of the newly-formed flaps, and had also prevented the saliva from trickling over and so excoriating the skin.

On May 28, by the advice of my colleagues, I made a further attempt to relieve the unsightly eversion of the lower lip, by excising another slip of mucous membrane within the mouth.

This last operation has certainly had a good effect in lessening the protrusion.

The boy has now for some months discontinued the use of his chin strap, and has no overflow of saliva, except occasionally when eating. He can whistle, and can completely close the mouth. The skin of the neck is free and healthy in texture, but marked by the lines of the incisions of the first operation.

Remarks.—The chief point of interest in the case is the benefit conferred on the boy by the first operation.

The result of operations for cicatrices in this region are generally by no means good, and the late Mr. Holmes Coote says in reference to operations for contractions of this kind: "I express the opinion of most surgeons of experience of the present day,

in affirming that hitherto all such operations have proved failures ; and for this simple reason, that wherever an incision is made a new cicatrix must be formed, and this new cicatrix will undergo precisely the same process of contraction as the former, which it was intended to alleviate."

Possibly Mr. Holmes Coote, in the above remarks, alluded to more severe and extensive injuries than the case I have here related, for in this boy the cicatrix tissue did not occupy the whole of the skin of the neck, but extended only about half-way down. I was therefore able to cut in healthy skin when making my flaps. Nevertheless, the view that is expressed in the above quotation is perhaps a little too despondent ; because there must, in most instances, be some healthy skin at the sides of the neck or at the upper part of the chest, and by means of sliding operations it will generally be possible, as in my own case, to fill up a considerable gap.

By using the triangular flaps the difficulty which Mr. Holmes Coote feared as to the contraction of the new cicatrices is got rid of completely. The lines of the new raw surfaces are so arranged as to be nearly horizontal, so that the contraction, if any, will only take place in the horizontal direction, and will therefore be of no consequence so far as the chin and sternum are concerned ; and the excessively distensible and loose quality of the skin of the neck allows of abundance of material from the sides to fill up deficiencies in front, so that horizontal contractions will have the effect of dragging the skin of the sides towards the front, and will therefore tend to separate the chin and sternum. *Quod erat faciendum.*

The principle of employing two triangular flaps dovetailed into one another is applicable to a large number of plastic operations in which a vertical contraction is objectionable. Thus the deformities of the eyelids from burns, *e.g.* ectropium, may often be remedied in this way, and I have already brought a case in point before the readers of the *Practitioner*. (See article on Ectropium, in *Practitioner*, Dec. 1872, pp. 325, 326.)

It appears to be very important to take with the skin a sufficient thickness of subcutaneous areolar tissue, and also to make the points of the flaps sufficiently obtuse or rounded to obviate the possibility of their blood-supply being deficient and slough-

ing consequently taking place. If possible there should be no points; rounded angles are better. Another practical point is to avoid undue pressure in applying the dressings. I fear that in applying the pads to the chin after the first operation the pressure had something to do with the slough that separated at one point. It is always well to allow for some amount of swelling of the edges afterwards, and hence any bandages and pads should be rather too slack than too tight at the first dressing. Of course, if the edges or any part of them mortify, the styptic colloid is useless or injurious at those parts; but in most cases it will not be necessary to remove it altogether, and it almost always favours union by the first intention in part of the wound. If suppuration or sloughs occur, it is easy to take away the crust from those parts at which the mischief is occurring, and leave the rest untouched.

Some have proposed and practised gradual extension by means of apparatus in these cases, and I am not prepared to deny that in suitable instances some benefit may be obtained in this way; but it is extremely difficult to adapt any kind of extending apparatus to the head and neck of a child 6 or 7 years of age. After an operation, some extending apparatus, such as a simple elastic band over the head and under the chin, may be of advantage; but even then it must be worn for a considerable period. Under the most favourable circumstances only a very imperfect result is obtainable; but if *some* amelioration of the patient's condition is secured, it will repay the trouble bestowed upon the case.

REMARKS ON CERTAIN RECENT PAPERS ON THE ACTION OF ALCOHOL.

BY DR. ANSTIE.

PART II.

THE influence of alcohol in reducing temperature is a fact so contrary to preconceived ideas that until within the last few years it was never even suspected. The first decided demonstration of it was given by Lichtenfels and Fröhlich in 1852. Their experiments were made with moderate doses upon healthy persons, and the reduction obtained amounted only to some fractions of a degree (Centigrade). In 1866 Tschechechin, and in 1867 Möller, experimented on animals with larger doses, and obtained more considerable reductions; and in the meantime Ringer and Rickards (1866) had independently carried out their well-known series of experiments upon healthy and also upon sick persons, fully demonstrating the general fact of the lowering of temperature, and going more into detail as to the varying results under different circumstances.¹

The attention of Professor Binz was drawn to the subject while he was still unaware of the researches of Ringer and Rickards; and at his instigation M. Cuny Bouvier made a series of experiments, in the laboratory at Bonn, with very decided results. On healthy men and on animals alcohol in moderate doses produced a small, and in larger doses a considerable, reduction of heat. Moreover, a dog which had been thrown into a pyrexial state by the injection of pus into the subcutaneous tissue, had its abnormal temperature markedly reduced by three considerable doses (each about 1 drachm

¹ *Lancet*, 1866, vol. ii.

absolute alcohol, with water), the lowering amounted to more than two degrees in all. The alcohol was then discontinued, the temperature rapidly rose again, and went on rising till death.

Since that time the subject has made great advances under the direction of Binz¹ and his pupils, especially Bouvier, who in 1872 published a more extended work upon alcohol,² in which the temperature question was again summed up. Finally, at the recent meeting of the British Association, Professor Binz, though necessarily in somewhat popular style, laid down, as the definite result of recent experimentation, that alcohol always reduces temperature, but that this effect was most readily perceptible in fevered persons, in whom the bodily heat was much above the normal line. In this respect, be it remarked, he differs from the conclusions of Ringer and Rickards, who did not find such marked reductions of heat in fevered as in healthy persons.

As the one notable exception, in its results, to all recent inquiries that have been made in a truly scientific manner, I must mention the inquiry of Dr. Parkes (1871).³ That eminent observer failed to find any constant reduction of temperature from the use of alcohol; indeed, on the whole, his results seemed to point rather the other way. We shall return to his papers presently; but I must mention that Parkes's experiments were made only upon a *healthy* man previously very temperate in the use of alcohol.

I cannot help pausing here to remark on the singular and even grotesque course which English popular medical opinion has taken upon this question of alcohol in pyrexia, since the death of Todd. His observations, which were doubtless inexact (for medical thermometry did not in his time exist), have been declaimed against in every variety of tone; but the special reproach against them has always been that they were "not truly scientific." Loud as this outcry has been, I am not aware that (before Ringer and Rickards) a single English medical

¹ Ueber die antipyretische Wirkung von Chinin u. Alkohol. Virchow's Arch. B. 51, 1870.

² Studien über Alkohol. Bonn, 1872.

³ Transactions Royal Society.

observer (one or two of Todd's pupils excepted) had ever put to the simple test of the thermometer Todd's main allegation—that alcohol reduces febrile temperature by its direct action. On the Continent, however, things have gone very differently. In France, the observations of Behier and of several of his most distinguished pupils, have affirmed the power of alcohol to reduce pyrexial temperature from clinical evidence;¹ and Demarquay (as far back as 1859) had shown by physiological experiment that alcohol was on the whole a cooling agent. In Germany, as we have seen, there has been a *catena* of evidence to this effect, ever since 1852, and a great impulse was given to the inquiry by Todd's "empirical facts," as Binz calls them—which, it was felt, demanded an accurate sifting. The practical result is sufficiently remarkable. That very portion of Todd's practice which has excited the fiercest condemnation in this country, viz., his administration of *very large* doses of alcohol, in conditions of *high* pyrexia, turns out to agree best with the results of physiological investigation, and has in practice been endorsed by such eminent physicians as Liebermeister, Binz, Socin,² and many others. The Franco-German war gave abundant occasion for testing this matter; and it was fortunate that Professor Binz received a high medical military post, and that such men as Socin and others, who also filled important medical posts in the war, were willing to carry out his plans with vigour. Those who are acquainted either personally or through his writings with Professor Socin, are aware that a more accomplished observer is not to be found in Europe; it need, therefore, hardly be said that his war-experiences of the treatment of pyæmia, erysipelas, &c., possess an uncommonly high value.³ It is therefore a matter of much importance that we find him stating that in the severe septicæmic wound-fevers he not only employed quinine in the enormous daily quantity of 6·7 grammes (90 to 105 grains), but at the same time gave three bottles of wine every day. Under this treatment he saw many unexpected improvements and even recoveries; and he

¹ See, especially, Godfrin, "De l'Alcohol," &c. Paris, 1869.

² Professor of Surgery in Basle.

³ *Kriegschirurgische Erfahrungen: gesammelt in Karlsruhe, 1870 and 1871.* Leipzig (Vogel), p. 27.

declares that the wine not only increased the lowering effect of the quinine on the temperature, but that it also much moderated the toxic effects, *i.e.* the "cinchonism."

Erysipelas was also a disease that came very largely under Socin's observations at Carlsruhe; and he speaks here in a different manner. He especially remarks that quinine had but little or no effect in reducing temperature, and all remedies appear to have been unavailing to reduce the temperature very markedly. Nevertheless, the patients supported the high temperature particularly well; and he attributes this, at least in part, to the administration of three to four bottles, daily, of champagne and sherry mixed.¹

I think it is not too much to say that such experience, related by a man like Professor Socin, of the treatment of diseases like erysipelas occurring in wounded men of previously healthy life, is sufficient to render completely worthless the whole mass of criticism which was levelled at Todd for his supposed enormities in the way of dose. Here are certainly all the conditions of a so-called "sthenic" febrile disease; yet they were *habitually and successfully* treated with enormous doses of alcohol—such, indeed, as Todd never, or almost never, gave. I do not at all say that it decides the controversy as to the treatment of fevers by alcohol. But it certainly, in my opinion, sweeps the arena of discussion perfectly clear of the *à priori* arguments which have been urged by the so-called "moderates" during the reaction since Todd's death, and leaves it open for a perfectly new discussion of the subject.

To return, however, to the subject of the cooling influence of alcohol in pyrexia. The researches of Binz have gone far to prove that this is not exerted through the heat-regulating centres. In his papers in Virchow's Archiv. (1870) will be found the record of experiments in which the possible interference of such a cause was elaborately provided against. And the general result of his inquiries is to reinforce, more strongly than any other researches have done for a long time, the opinion that alcohol, while itself oxidised within the blood with great

¹ Op. cit. p. 85. "In Carlsruhe . . . liessen wir solche Fiebernde, mit dem besten Erfolg, 3-4 Flaschen eines Gemisches von Sherry und Champagner täglich zu nehmen."

rapidity, hinders the oxidation of the tissues. Hence it can be easily understood that the effect of alcohol in lowering temperature would be seen most plainly in the febrile diseases, where the tissues are being violently consumed, while the supplies of ordinary nutriment are necessarily very small. Such, in fact, proves to be the case, and Dr. Binz has recently informed me (last August) that whereas in smaller doses, *e.g.* one ounce of wine or half an ounce of brandy, it is usually not possible to obtain a reduction of more than a few decimal parts of a degree, and the dose must be frequently renewed to maintain the effect, he has found that the influence of a very large dose has sufficed to produce a reduction amounting to several degrees. Whether this be, on the whole, the best method of relieving pyrexia by means of alcohol, is a question which will be found more fully investigated in my new researches, which will shortly be published.

6. Another question of great moment which also requires the attention of the profession, is whether alcohol reduces the excretion of urea, and the special relation which this reduction bears to the existence of the fevered state. The experiments of Dr. Parkes in 1871 have, since that time, constantly been cited as evidence against the possession, by alcohol, of any power to diminish the urea. It is therefore very necessary that we should remember accurately what these experiments proved. The subject of them was a very healthy soldier of unusually temperate habits. He was submitted to two successive investigations, the first being made with alcohol diluted with water, and with brandy; the second being made with Bordeaux wine: but as the results appear to have been substantially the same in both series of trials, we may take the first, in which the effects of alcohol and of brandy were successively tested. The result may be simply stated as altogether negative: no reduction of urea elimination was observed. So far, then, as the observations go—and I need scarcely say that all Dr. Parkes's work of this kind has deservedly the highest possible reputation for conscientious care and accuracy—all one can say is, that they stand in marked opposition with the results obtained by a number of very eminent authorities. But what are the facts really shown? Merely, that in a man who was in good health, *and was taking a normal*

amount of nitrogen in his food, alcohol, even in considerable quantities, did not lower the urea-discharge. But under these circumstances all the discharged urea is to be put to the score of *ingested* nitrogen, not to combustion of nitrogenous tissues of the body : and I do not know why it should be expected that the alcohol should have lowered the rate of conversion of food-nitrogen into urea. The man experimented upon was a particularly healthy person, taking proper physical exercise ; under these circumstances it was not probable that his organism would find any difficulty in passing its nitrogenous food-supplies through the oxidation changes necessary to form urea.

Those who have argued that alcohol can supply (by its own oxidation within the body) the place of other oxidation processes have usually supposed that it was the oxidation of tissue which would be replaced, not the oxidation of *other foods simultaneously taken*. Now, when a man is in health and is taking a moderate quantum of nitrogenous matters and a fair amount of exercise, the very lightest and easiest part of the oxidation work of his organism would surely be the disposal of the nitrogenous food-supplies. I grant that it is an interesting fact that even such large doses (12 ozs. of brandy per diem to a previously very temperate man) did not avail to perturb his digestion and assimilation more than was actually the case ; but as it is expressly stated that up to the end of the experiments, the man, though losing appetite, continued to eat his regular allowance of food, it was evident that disturbance had not proceeded very far ; while, on the other hand, we may suspect that things would have changed had the experiment been carried on much longer, for there was a significant increase in the free acidity of the urine.

It is not, however, with the aspects of alcohol as an article of food for the healthy that I have to do on the present occasion. The bearing of my present remarks is towards its influence in diseased states, and especially in distinct pyrexia. And in this respect I may be permitted to say, decidedly, that though the evidence may be somewhat defective—as Dr. Parkes terms it—as far as it goes its bearing is all in one direction. It is well known that a number of pyrexial diseases—probably all in which there is any considerable elevation of temperature for more than the shortest periods—the elimination

of urea is increased, and this increase reaches a very high degree in the case of the more formidable febrile diseases; and to Dr. Parkes himself medical science is under the deepest obligations for his demonstration of this fact. Now, in the case, let us say, of typhus fever, in which for many days (at the early part of the disease especially) the amount of nitrogenous food that the patient can take is exceedingly small, we find that during that very period the urea-excretion is *double*,¹ that observed during the fourth week when the patient is sitting up and eating his fill of meat. Such an extraordinary consumption of nitrogenous tissue as this fact implies, must certainly be in itself a perilous matter when we consider the sources from which it is in all probability derived. The muscular tissues must certainly supply a very large proportion of it: and there are good reasons for thinking that the heart-muscles suffer heavily in severe cases with very high temperature. It must then unquestionably be a desirable object to reduce this waste of nitrogenous tissue, and the question is, can alcohol effect this purpose?

To speak now of the general results of my own experience, which will be given in detail hereafter, I have procured evidence of the most positive description that this limitation of urea-waste is effected by alcohol, though under certain conditions. The first case in which I tested this was in 1866; a typhus patient, while taking 6 ozs. of brandy daily, was found to excrete only 385 grains of urea daily. Since that time I have repeatedly made observations with similar results. Nor is this merely a momentary damming-up, so to speak, of the tide of urea-waste; the excretion remains at a remarkably low level throughout, and the defervescence is not followed by any increase in the total daily urea-excretion.

Dr. Parkes mentions a circumstance which occurred in the course of his experiments, and which seemed adverse to the idea that alcohol could reduce or prevent febrile rise of temperature or febrile excess of urea-excretion.² The soldier on whom he was experimenting caught a slight catarrh about the twelfth day of

¹ See Dr. Buchanan's researches on sixteen typhus patients, mentioned in his article in Reynolds's "System," vol. i.

² Vide *Lancet*, vol. ii. 1867, p. 358.

the process, at a time when he was taking six ounces of absolute alcohol, diluted with water, in the day ; nevertheless he had a rise of temperature to 100°, and the urea increased considerably, as much as 92 grains. Dr. Parkes thinks that this shows that alcohol had no power to restrain the febrile movement, either as regards temperature or the excretion of urea. But not to mention the fact that, for aught we know, the alcohol did hinder these occurrences somewhat, it is to be remarked that the same patient had a second catarrhal outburst during the next period of the experiment, when he was entirely without stimulants ; that on this occasion both temperature and urea-excretion rose higher than they had done during the brandy-period ; that on the resumption of brandy the urea-discharge again declined ; and finally, that when he had again left off stimulants for three days the urea-discharge was about as high as it was under the brandy. Surely, this does not amount to evidence against the power of alcohol to reduce temperature and urea in fever? Remember, the highest temperature spoken of is 100°·7, and the largest urea-discharge, in twenty-four hours, is about 630 grains. It is not by these trivial fevers (ephemeral catarrh, and the like), as I submit, that the question of the anti-pyretic influence of alcohol can be tested at all adequately.

I cannot too strongly impress on my readers that the foregoing observations are not intended at all as a statement of my opinions as to the amount of value which is to be attached to the use of alcohol in disease. They are only directed towards the rectification of certain current errors respecting *particular facts* in the physiological and clinical history of alcohol. No summary of my present opinions on the general subject can be given till my later researches have been published.

Reviews.

The Training of the Mind for the Study of Medicine. An Introductory Address delivered at St. George's Hospital, by R. B. CARTER, F.R.C.S. London: Baillière, Tindall, and Cox, 1873.

It is not surprising that those who had the pleasure of listening to this remarkable address should have requested that it might be printed in full. It is perhaps not a very common event that the duty of delivering an inaugural address at a medical school falls upon one who is both a clear and independent thinker, and a perfect master of the English language.

Mr. Carter justifies the old-established practice of formally addressing the old and new students at the beginning of a winter session by arguments which are satisfactory in themselves; but the best justification of the custom is afforded by his own lucid and interesting manner of dealing with his subject. There are two sorts of inaugural addresses which have always appeared to us worse than useless: the first kind consists of a mere catalogue of the classes which the student will have to attend, supplemented by a few feeble observations on the advantages of imitating the industrious apprentice; the other kind is made up of a series of pompous platitudes respecting the dignity of the medical science and art, and seasoned with a few cut and dried quotations, by which the orator seeks to convey an idea of his own liberal accomplishments. We regret to say that the bulk of addresses to medical students are, in our experience, to be ranked under one or other of these heads. It has been a source of perennial wonder to us, that so few of these addresses really grapple in a serious manner with the actual problems the student has before him. He has not so much to learn, as in the first instance to find out how to learn; and, secondly, he ought to be told—what scarcely anybody ever does tell him—the real nature of the relations which in after life he will hold to his patients, and, further, to the public at large. As regards the learning how to learn, Mr. Carter's address is specially useful in teaching the student how to weave the threads of his preliminary education into the texture of his most special training for the medical art.

It is easy to see that Mr. Carter is so far a conservative as to lean, in the main, towards the time-honoured system of making classics and mathematics the backbone of general education; but, in the first place, he would enlarge the conventional sense of the word "classics," by including, and even making most prominent, the study of the English classics and the more important European languages; and in the second place he couples the physical sciences in a significant manner with mathematics. But even more important is the gist of his remarks as to the manner in which these studies may best train the mind for the subsequent acquirement of medical knowledge. These preliminary studies ought to issue, according to Mr. Carter, not in the mere deposition in the mind of little heaps of knowledge respecting their individual objects, but in the formation of four principal habits of mind: first, the power of careful observation of facts; secondly, the cultivated imagination by which we link together the known and the unknown; thirdly, the power to maintain a suspended judgment whenever certainty is unattainable; fourthly, and pre-eminently, the love of truth for its own sake.

In the examples by which Mr. Carter enforces the necessity of really careful observation of facts, there is much that is specially interesting to us in the bearing which it has on the special preparation of the student for that last and most difficult of medical accomplishments—the power to appreciate justly the effects of remedies upon disease. The cause which, more than any other, produces blind complacency in the administration of routine remedies on the one hand, or on the other leads the young practitioner to the hasty adoption of ill-tried remedial measures, is the incompetence in which his faulty education has left him, to observe truly the whole of the facts with which he has to deal. If he were content, for instance, to look upon a physical sign (such as fine crepitation) at first only as the exponent of a physical condition, he would not be so apt as he often is to conclude that it is the leading feature of the disease before him, and to rush to the conclusion that to that feature his remedies must be addressed. So, again, if on observing that a patient has a large and bounding pulse and a generally excited state of circulation, he would content himself with laying down these as mere preliminary facts which can only assume their true value when all the features of the case have been investigated, he would not so frequently rush as he does, to the idea of cardiac sedation as the one present object of treatment.

Under the heading of cultivation of the imagination, Mr. Carter has given excellent examples, and some of these are such good stories that they have already made their way into the pages of our contemporaries, lay as well as medical. It is

exceedingly necessary that the student should be taught to substitute the scientific "framing of hypotheses" (confessedly temporary in their objects) for the habit of blind guessing. Here, again, the bad systems of medical teaching, which have too widely prevailed, have been most mischievous in their influence on the style of treatment adopted by the practitioners trained under them. How constantly does one see that medical men, who would abhor the title of "theorists," are habitually guilty of that worst form of hypothesis which is in fact mere wilful conjecture!

Perhaps the worst examples of this fault are to be seen in the rashness with which practitioners often make up their minds that a particular case is "just like" some other, in which a particular treatment happened to succeed. The doctor has perhaps once had a case in which purgation with calomel did striking good in infantile convulsions: the chances are that in every subsequent case of infantile convulsions he will see a condition of things "just like" that of his first patient: whereas, if his imagination had been sufficiently cultivated to enable him to form rational hypotheses, it would certainly have occurred to him that the accumulation of fæces, acting perhaps on an unusually irritable nervous system, might have been the cause of fits in the first case, but that there was no good reason for thinking either that overloading of the bowels is a thing of universal occurrence, or that when it does occur it is particularly likely to cause fits. Perhaps his imagination might even have carried him the length of framing the hypothesis that some inborn feebleness of nervous constitution is at least as often the cause of infantile convulsions as any peripheral irritation so insignificant as to be immediately removable by one or two purgative doses of calomel.

Passing over the acute and sensible remarks which Mr. Carter makes upon the desirableness of learning how, in case of need, to maintain a suspense of judgment, we cannot but notice with great approval his observations on the *difficulty*, as well as the supreme importance, of obtaining a really truthful habit of mind. He is unquestionably right in saying that children and servants, and uncultivated persons generally,¹ are by nature not truthful, but deceitful. This is a fact of incalculable importance, and it is no slight reproach to the clergy, of all shades and schools of Christianity, that not one in a thousand of them ever states or is even aware of it, so far as can be known. On the contrary, the clergy teach, and society readily enough acquiesces in their teaching, that childhood and womanhood, and a lowliness of station which prevents the mind from being sophisticated by over-culture, are

¹ He might have added, with emphasis, most women, and all the "nervous" and "hysterical" ones.

cularly favourable to truthfulness. This is most mischievous, because it keeps out of sight the important fact that perfect honesty in mental vision and in the statement of facts is a difficult accomplishment for which there must be strenuous intellectual preparation. No doubt the average schoolboy, just turning into a medical student, would be greatly offended were he told that his particular weakness was a tendency to incorrect statement; but the statement would be true, for all that: although the unconscious character of the deceitfulness of such minds ought to mitigate moral opprobrium, and cause them to be treated with rational "straightening" measures, not with invitations to hysterical self-reproach. It would be a vast gain to us all if we recognised that untruthfulness, instead of being uncommon, is very common indeed, and nowhere more common than among a certain class of persons who believe themselves to be exceptionally "good," on the one hand, and among the conventional "honest schoolboys" on the other. [We fancy it was Thackeray who first had the courage to explode those last and most special humbugs.]

In the concluding part of his address, Mr. Carter made some striking remarks on the cultivation of the habit of sympathy in the best sense: that is, the power of seeing into the natures of different persons, and putting yourself, for the moment, at their special points of view. And he made the observation, which seems to us as original as it is certainly correct, that a truly observant man does not need to mix with every rank of society in order to understand the wants of all; for an attentive study of the very miscellaneous groups of persons who frequent our hospitals will afford him a perfect picture of human nature in all its types of strength and weakness, pride and humility, chivalrous courtesy and most sordid meanness. It is so; and we wonder that no one (to our recollection) has said this before. Certainly it is a fact of which the student too seldom gains any consciousness: but if he would only view the matter as Mr. Carter bids him view it, he might save himself many years of unavailing regret and needless difficulty in after life.

A Hand-book of the Theory and Practice of Medicine. By FREDERICK T. ROBERTS, M.D., B.Sc., M.R.C.P., Fellow of University College, London; Assistant Physician to University College Hospital, &c. &c. London: H. K. Lewis, 1873, pp. 1031.

It was certainly a bold undertaking on the part of Dr. Roberts to attempt the writing of a hand-book of the whole subjects conventionally included under the term "Theory and Practice

of Medicine ;" and, but for one circumstance, we should have pronounced it a rash one. That circumstance is the fact that the only existing work on general medicine, in the English language, which comes anywhere near the "Manual" size, was possibly a little out of date as regards some maxims and principles of pathology and treatment ; and that the English student who wished to get the general modern views of such matters *outlined* for him, did not know exactly where to turn. Whether it is desirable that students should be helped by any such machinery is of course an open question ; and we confess to have opened this volume with some trepidation as to the conclusions which we should have to form respecting the utility of the author's work. It is given to very few men to tread in the steps of Austin Flint, whose single volume on Medicine, though here and there defective, is a masterpiece of lucid condensation and of general grasp of an enormously wide subject. We do not suppose that Dr. Roberts hoped to equal Dr. Flint in these qualities ; indeed, in his preface he very modestly deprecates being judged by too high a standard ; but he may reasonably have expected to be able to furnish to the student, and also to the fully occupied practitioner, a good deal of recent information for which they might otherwise have to search over a wide field of medical literature. In this purpose we think he has, on the whole, succeeded ; and the volume before us is highly creditable to his energy and perseverance.

The introductory portion of the work before us, though undertaken from the best motives, would probably have been better omitted ; we refer to the sections on bed-side observation, on the etiology of diseases, and on semiology. Such topics really fall into the province of manuals for clinical clerks, and should thereon be handled with more fullness of detail and with more careful and philosophical thought, than was possible within the compass of twenty-five pages. We have not the same objection to offer to Dr. Roberts's next section, which is occupied with seven chapters on important leading questions on pathology, viz., Hyperæmia, Dropsy, Hæmorrhage, Inflammation, Hypertrophy and Atrophy, Degenerations, and Pyrexia. Upon scarcely any of these subjects is it possible to gain an adequate idea of modern knowledge and opinion in any existing text-book of medicine ; while, on the other hand, to remit the student to pathological treatises for this information, would be to run the risk of swamping his mind with too much histological detail. At the same time it must be confessed that these subjects, if treated at all, should have been treated more fully.

We pass now to our author's consideration of General Diseases ; and as the subject of acute specific fevers naturally comes first, he introduces a preliminary chapter on Contagion,

Epidemics, on the Hygienic Treatment of Contagious Fevers, and the Prevention and Limitation of Epidemics; with an appendix chiefly taken up with the subject of thermometry, which will be of considerable use as an introduction to the study of individual fevers.

The continued fevers, which come first, are well described, though we are rather startled to see that among the rules and cautions for the use of alcohol in typhus, the author represents it as unsuitable in cases with very hot and dry skin. Considering that alcohol is established by important recent researches to be a direct reducer of febrile temperature, we certainly think that Dr. Roberts ought not to have given this routine direction. Under Scarlatina we are rather surprised to see that the very important occasional sequel to that disease—atrophy of the optic discs—is not mentioned, though the article generally is a good one: indeed, the acute exanthemata are, as a rule, well described. In the article on Erysipelas we notice with surprise that plethora is described as a predisposing cause; this statement may doubtless be found in routine treatises, but is surely destitute of any foundation.

We think also that much more consequence should have been given to the subjects of throat erysipelas and to the absolute necessity of very free stimulation in such cases.

In Section II., on Constitutional Diseases, we notice that in the article on Rheumatism the author adheres to the alkaline treatment, and states that his experience in the Liverpool Hospital showed that this distinctly tended to avert heart complications.

We observe, too, that he deals with the myalgic group of affections as a variety of chronic rheumatism. As regards gonorrhœal rheumatism, we observe that he gives no hint of concurrence in the idea that this disease is related to the pyæmic infections.

Dr. Roberts is seen to advantage in dealing with local diseases, and it is evident that chest diseases are his strong point. The introductory section, on the physical examination of the chest, is carefully done, and would form a good text-book for those teachers who are now so frequently appointed in medical schools to initiate students in the extremely difficult art of appreciating physical signs. We are rather surprised, however, to find whooping-cough and influenza treated as local diseases; they should have been ranked among epidemic general diseases.

The article on Bronchitis is of course very good, as Dr. Roberts has already given proofs of his special knowledge of this subject in his treatise in "Reynolds' System of Medicine." The article on Pneumonia is also well done, though briefly, considering the frequency and importance of this disease. One of the test-articles, of course, in any work of general medicine, is that on Phthisis; and although Dr. Roberts has not had space to do

himself full justice on this subject, he has compressed a large quantity of information into a small compass, and told his story well.

The section on Diseases of the Circulatory Organs is opened by a preliminary discussion of the clinical phenomena connected with this system which will be extremely useful to students and practitioners. The article on Pericarditis is very good, but we were disappointed to find Dr. Roberts so conservative with regard to the operation of tapping, which assuredly ought not to be limited to a few very extraordinary cases. The chronic diseases of the heart are very carefully dealt with, but the article on Thoracic Aneurisms is much too short.

We should have been glad to run through each of the principal sections on local diseases as dealt with by Dr. Roberts, but our space will not permit us to enter on greater detail. We have much pleasure in closing this hurried notice, with the expression of our sense of the author's conscientious anxiety to make his work a faithful representation of modern medical beliefs and practice. In this he has succeeded in a degree which will earn him the gratitude of very many students and practitioners who must be driven to despair by the apparently limitless expansion of the field of medical knowledge. In the successive editions which will probably be called for, Dr. Roberts will have the opportunity of further increasing the value of a work which is already a remarkable evidence of industry, experience, and research.

Essays on Diseases of Children. By WILLIAM HENRY DAY, M.D., M.R.C.P., Physician to the Samaritan Hospital. London: Churchill, 1873.

THIS little work is not a manual of children's diseases, but a collection of essays on particular subjects in that department of medicine. It is welcome, as must be every honest and careful attempt to improve our knowledge of infantile diseases, for it must be acknowledged that we are still very much in ignorance about them. The seven essays—on Debility in Children, "Remittent" Fever, Case of Obscure Cerebral Disease, Headaches (two papers), Laryngeal and Tracheal Irritation, and on Croup—have already been read before medical societies: but we presume that the Introductory Remarks on the Study of Children's Diseases, which form the first and longest chapter, are newly written. The general style of the book indicates that Dr. Day is a man of considerable practical experience and an earnest observer; we therefore looked with some interest to what he has to say about the peculiarities which distinguish the study of children's diseases from other departments of clinical medicine. We cannot say, however, that this general subject is treated so satisfactorily as

are several of the special ones which are dealt with in subsequent chapters. The reader would infer from it that the author is a sound practitioner, but he would hardly do justice to the qualities which Dr. Day shows elsewhere on a mere perusal of his introductory remarks.

The chapter on Debility in Children, though much too short, does really describe an important and little noticed class of cases. We were afraid, at first, that Dr. Day was, in fact, going to describe cases of rickets of which he had not recognised the rachitic character, but we did him an injustice, and will say that his remarks apply to a state of things rather difficult to recognise when first seen, but which it is very important that every practitioner should be familiar with. The fourth chapter is that which, partly from accidental circumstances, has most attracted our own attention, and is certainly of deep practical interest. It narrates a case of cerebral disease in a child in which the symptoms were in the highest degree formidable as well as obscure, and in which a perfectly unhopd-for good result has occurred, in consequence of a patient and persevering treatment. The case is that of a child of 2½ years, who was attacked first with laryngeal irritation, after which he remained in a very weak state for two or three months, and then developed formidable head-symptoms, including severe pain, sickness, and squinting, with grave disturbance of consciousness; in fact, there seemed every reason to believe that he was suffering from tubercular meningitis. Then there followed impairment of speech and right hemiplegia—(the author thinks the speech-affection was only articulatory, but we should much doubt if there were not true aphasia mixed up in it). Next, the child recovered complete command over his tongue, lips, hands, and arms, but remained with somewhat impaired memory. This was more than a year and a half after the commencement of the symptoms; but since that time we have reason to believe further progress has been made, and that entire recovery may be expected. The whole case is unquestionably very instructive, and ought strongly to enforce the *nil desperandum* doctrine in regard to infantile cerebral affections. We have just one suggestion to throw out—was this one of those instances of general rachitic constitution in which a real hydrocephalus—that is, a passive exudation of serum—takes place and may again disappear? We must say that many symptoms look like it, especially the fact that the first serious trouble was “laryngeal and tracheal irritation.” The shape of the head is not mentioned, but the author’s description of the general state is highly suggestive of the rickety state. The mention of the word hydrocephalus reminds us that we have often intended to challenge our authorities on children’s diseases to publicly

rectify an error which is still allowed to exist in a phraseological form, and does, we believe, incalculable mischief—we mean the belief in a substantive disease called “chronic hydrocephalus.” The word is intolerably bad. In the first place, it suggests some relationship to the perfectly different acute hydrocephalus—the specific tubercular meningitis; and, secondly, it entirely misrepresents the facts of the chronic cases, in which the effusion is always a secondary and not a primary affair; and the real object of our efforts, dietetic and medicinal, ought to be to alter a general constitutional state (nine times out of ten the rachitic) which lies at the bottom of the mischief.

The main cause of the remarkable credulity which still prevails as to this substantive hydrocephalus, and of the tenacity with which many practitioners cling to the old treatment (with calomel, &c.), is the complete novelty of the scientific knowledge about rickets. It is not too much to say that Sir W. Jenner *created* that knowledge some twenty years ago: the proof of which may be given in a way which is alike amusing and instructive. It is a fact that in the first two yearly reports of the Ormond-street Children's Hospital *no mention is made of rickets* among a formidable list of diseases under which the out-patients were said to suffer. Then came the appointment of Sir W. (then Dr.) Jenner, and very soon it became apparent that rickets is one of the chief, if not *the* chief, morbid conditions with which a children's hospital has to contend! Since that time a silent influence has pervaded all the hospitals in London, and without any formal recantation of errors it is singular to observe how constantly we now ascribe the over-large head, with unclosed sutures, &c., to the rachitic diathesis, and how uncommonly seldom the existence of fluid in the skull enters into our practical considerations! And surely, even Dr. Day himself does not quite clearly show that his “laryngeal and tracheal irritation” (chap. vii.) is anything but a variety of the laryngismus stridulus which is one of the commonest appendages of the rickety diathesis?

We can much commend Dr. Day's exceedingly practical chapter on Headaches in Children. Much as has been said on this subject, there is always more to say: and we feel sure that the questions connected with it are far from being yet solved; in the meantime Dr. Day's descriptions will be very useful.

Finally, we must say a word as to our author's way of dealing with the term Infantile Remittent. We are glad to see that Dr. Day is not at all inclined to believe that there is a specific disease which might answer to this name. On the contrary, excluding a certain few cases of misinterpreted tuberculosis, &c., he assigns the bulk of the cases which get labelled as “remittent” to the category of ill-developed typhoid, in which, no doubt, he is perfectly right. We are not inclined to go with

him in the proposal to retain the word with the understanding that it implies only a modification of typhoid fever. The word has done, and is doing, such incalculable mischief, it has proved such a refuge for gross ignorance, and such an easy cloak for atrociously bad practice, that we consider that it ought to be most carefully excluded from every medical treatise; and the compiler of every work on children's diseases is especially bound to explain that "infantile remittent" has exactly the same kind of existence as was enjoyed by the great Mrs. Harris.

Clinic of the Month.

Bloodless Operations on the Penis.—Mr. J. T. Clover writes that those surgeons who are disposed to make their operations bloodless will find a clamp that he has had constructed for him by Mayer and Meltzer, of Great Portland Street, very convenient for amputation of the penis, circumcision, and the removal of warty growths. It consists of two discs of vulcanite, four inches in diameter and one-eighth of an inch in thickness, connected by a rivet which allows one to slide over the other. A V-shaped notch is cut from the circumference to the centre of each disc, but in opposite directions, so that they form together an opening large enough for the penis. By sliding one disc over the other, the opening is contracted, and when the penis is sufficiently compressed to stop the passage of blood, a binding-screw fixes the clamp.

Mr. Clover used to tie the penis with a piece of cord or red tape, and protect the pubes from any drops of blood by a piece of waterproof with a hole in it, but about six years ago it occurred to him that the instrument he has described would act both as the most convenient form of tourniquet and sufficiently protect the pubes, and on making use of it he found that it had the further advantage of retaining the penis in a position convenient for the surgeon.

Whilst writing on this subject, he desires to recommend a method of applying sutures suitable in all cases where the skin is very thin and abundant, but especially after the circumcision of boys. The plan is to use a glover's needle and fine waxed silk, and puncture as near as possible to the cut edge, and then draw the suture as tightly as if tying an artery. There is no need to tie the second half of the reef-knot. The skin will give way sooner than the knot. These sutures come away in removing the dressing on the second or third day, without any need of using scissors or forceps—instruments that are regarded with terror at these times. Mr. Clover has often been called to give chloroform while the surgeon removed the sutures from a cir-

cumcised penis, he having been unable to keep the patient sufficiently steady without it. In order to pierce the skin so near to the edge it must be held with forceps, and if they have a notch at the points the skin is held so steadily that a common sewing-needle may be used. (*Lancet*, Nov. 8, 1873.)

Treatment of Poisoning by Laudanum with Subcutaneous Injection of Liquor Ammoniae.—Mr. Hartley, of Warwick Square, London, in a letter to the *Lancet*, states that he was called to a man at 9.30 A.M. on the 28th October, 1873, and found him quite insensible. Pupils contracted to a point, face much congested, and breathing laborious. Mr. Hartley was shown a bottle containing a little laudanum, and which had been found near the patient. A stomach-pump would have been used had his friends not objected. Sulphate of zinc was given in large doses frequently, but, as it did not act, mustard was combined with it, and produced free vomiting. He was kept constantly walking about, and partook freely of coffee. Powerful purgative medicine was administered, but without effect, during the first three or four hours that Mr. Hartley was in attendance, and half a drachm of tincture of belladonna was given hourly. By 2.30 P.M. the patient was able to converse with his friends, and explained that he had taken half an ounce of laudanum, which he had procured in small quantities at five different shops. At 4 o'clock, all the symptoms having subsided, he was allowed to go to bed. At 6 P.M., the bowels not yet being open, an enema of soapy water was administered, which acted. He was seen again at 10.30 P.M., when he seemed not quite so sensible; pulse 108, full. At 11.30 P.M. Mr. Hartley found him a great deal worse; there was lividity of the face, pulse thready, great difficulty of breathing, total insensibility, pupils contracted, and skin cold. Mustard poultices were applied over the heart, to the back of the neck, and the calves of the legs. He took frequent doses of sal volatile, but with little benefit. Mr. Meates and Mr. Hartley decided to inject subcutaneously one drop of liquor ammoniae fortissimus, diluted with four drops of warm water, after which he was kept walking about the room. An hour later the subcutaneous injection was repeated. The pupils then dilated a good deal. The administration of coffee and sal volatile was continued, and half a drachm of tincture of belladonna was also given for a dose. At 2 A.M., finding the pulse very flagging, and there being greater difficulty in keeping the patient walking, the liquor ammoniae fortissimus was injected as before. His eyelids began to droop very much, but, notwithstanding this, at 2.50 A.M. he asked for his watch, to wind it up. At 3.8 A.M. he sank on his knees and expired. (*Ibid.*)

Fallacies and Failures in Antiseptic Surgery.—Mr. Lund, of Manchester, whilst maintaining that the antiseptic method of treating wounds, large abscesses, and similar cases is right in principle and successful in practice, points out a series of fallacies and the causes of failures that occur in its application.

1. It is a fallacy, he says, or an erroneous doctrine in antiseptic surgery, that it is the agent employed, and not the result produced, which must be closely watched.

2. It is a fallacy to assume that the success or failure of the system is to be measured alone by the absence or presence of suppuration.

3. It is a fallacy to look upon the secretion and expulsion of pus as so much broken-down tissue produced by the removal of previous induration, the reduction of swelling, and the clearance of the general system of some peccant matters.

4. It is a fallacy to regard the admission of air to wounded portions of the body, unless the air be filtered and disinfected of its septic particles, as a matter of light importance, and to omit to note the subtle influences which travel with unmeasured rapidity along the atmospheric wave.

5. It is a fallacy to ignore the possible contagion of septic matters from one wound to another, from one person to another, from tainted instruments and soiled dressings, and thus to neglect the most scrupulous cleanliness in the treatment of wounds by quick removal, from within their range, of the minutest portions of the products of putrefaction.

6. It is as fallacious as it is inconsistent, to attribute on the one hand to atmospheric causes the presence of pyæmia, septicæmia, erysipelas, and the like, and yet to take no precautionary measures which can be proved to be efficient to neutralise these ill effects in the treatment of wounds. (*British Med. Journal*, Oct. 1873.)

Enlargement of the Bronchial Glands.—Dr. Gueneau de Mussy observes that, though often noticed in post-mortem examinations, this affection has only been described both in children and adults in its most advanced stages and most severe forms, when the windpipe, by the pressure which it undergoes, is narrowed, and a great obstacle to the passage of air is produced. In many cases, however, this enlargement of the bronchial glands is not attended with the symptoms which belong to the worst degree of the disease, yet this milder and usually unknown form gives rise to peculiar troubles of respiration; it is attended by peculiar symptoms, and it may be detected during life by positive signs. The signs are, that there is some dulness on percussion behind the upper part of the manubrium of the sternum, the sterno-clavicular and the first sterno-costal joints,

and the inner part of the first two intercostal spaces. (Behind, the laminæ of the first four dorsal vertebræ correspond to the same organs.) There is also weakness and roughness of the respiratory murmur in the whole or part of one lung, according to whether the main bronchial tube be compressed, or only one of the secondary divisions. Near the origin of the bronchial tubes a rough sibilant or rubbing sound is frequently to be heard, and also an expiratory souffle, which is only the tracheal sound transmitted by the enlarged glands. The expiratory souffle is remarkable in the circumstance that sometimes the movements of the body or neck may modify it. The cough is dry and obstinate, and is sometimes attended with a long whistling inspiration like whooping-cough. There is slight dyspnœa, sometimes recurrent and assuming an asthmatic form. Aphonia is occasionally present. The treatment should consist in the administration of mineral waters containing chloride of sodium and arseniate of soda, such as those of La Bourboule; or chloride of sodium and iodine, as Kreuznach, Salies, and Salins, and in certain cases sulphurous springs, may be efficaciously prescribed. The use of sodium internally and externally is also indicated. (*British Med. Journal*, Nov. 8, 1873.)

Treatment of Club-foot.—Mr. W. Parker, late surgeon to the North Devon Infirmary, writes a paper with the object of showing the physiology, character, and cure of the deformity without recourse to the knife, or confinement to the bed in any case, if undertaken under five years of age or so. Four well-marked cases have proved the truth of these observations under the use of the same tin-plate splint, which is fitted to the heel, &c., whilst the stocking and shoe can be worn as usual, and no pain is produced by the process of cure.

1. The physiology of club-foot consists of deformity of the foot being turned inwards and upwards in the various forms recorded, which are unnecessary to enter on here, whilst the heel is drawn upwards and the tendon of Achilles is tense as a whip-cord in *all* cases.

2. The character consists of no disease of any part, whilst the stronger muscles (gastrocnemius, &c.) overcome the actions of the extensors of the toes; thereby the limb is deformed, and continues to be so, until one, or even four tendons are divided, and extensor instruments with screws and pads are used to keep up the separation of the ends of the tendons. This process is attended with extreme pain and continued confinement in bed for a month, or six weeks in some cases.

3. The cure of the deformity is effected, whilst the bones and tendons are (easily in infancy, without pain) put and kept in their natural position, and indicated by the sole of the foot

being kept flat on the ground for two or three weeks, whilst the extensor muscles of the toes effect their natural action without being impeded by the stronger muscles of the calf of the leg.

The four cases fully prove that the stronger muscles had overcome the weaker, and the treatment has proved that the weaker muscles had been strengthened, and the feet became natural in their actions. If the above theory and practice cannot be refuted by more intelligible language, Mr. Parker thinks it may be still shown that the treatment at present pursued at the Orthopædic Hospital, and that generally adopted, with irons on either side of the limb, is devoid of encouragement and perfectly unnecessary. In regard to present plans in infancy, the surgeon may or may not be consulted by the parents, the surgical instrument maker being sufficiently instructed by drawings of the apparatus for club-foot in all medical periodicals and circulars, so that no particular credit of invention is due to the manufacturer. Furthermore, the hinge in the apparatus at the ankle is absurd, whilst perfect quietude of the joint is requisite for the cure.

The last case, of a little girl, aged four years and four months, had been under treatment at the Bath United Hospital, with two wooden splints and a calico bandage, for some three months. Subsequently a surgical instrument maker had made the ordinary irons, which were worn for two years, when the deformity was greatly increased by the instrument not fitting the parts, which would require amputation, as the foot would have been useless. Another matter—removal of the irons every night and their application every morning contravene the cure, whilst the tin-plate splint is constantly kept up for the three weeks or so.

Anticipation and Treatment of Post-Partum Hæmorrhage.—Several interesting communications on this subject appear in the *British Medical Journal* during the months of September, October, and November. Dr. Whittle (Sept. 27), in a paper read before the Obstetrical Medical Section, states that the probable occurrence of post-partum hæmorrhage in any case of parturition can be diagnosed beforehand by the pains being of a peculiar character, strong and quick and ceasing suddenly, their duration being from forty to fifty seconds, with intervals of five or six minutes. The cause of the hæmorrhage is obvious: the uterus contracts vigorously and relaxes completely; after the child is born a relaxation follows; one or two sharp pains expel the placenta with a gush of blood, and the uterus again relaxes, continuing the same tendency which existed before the delivery of the child. In such cases Dr. Whittle considers he is justified in interfering, and he accordingly gives a teaspoonful of a liquid extract of ergot twice the Pharmacopœia strength as.

soon as the os is fully dilated. If this does not improve the character of the pains at the end of an hour, he repeats it, but rarely finds it to be requisite. In primiparæ care must be used not to administer the ergot till the soft parts are pretty well dilated, as well as the os uteri, and much smaller doses should be administered. Dr. Lombe Atthill, commenting upon this paper, remarks that the causes of post-partum hæmorrhage are—1. Exhaustion of the uterus. 2. Exhaustion of the nervous system of the mother. 3. Too rapid emptying of the uterus. 4. Failing to give support to and the injudicious manipulation of the uterus after the expulsion of the placenta. Apart from these there may be other unusual causes, as laceration of the cervix and the resistance of a tumour. In regard to treatment, Dr. Atthill thinks the uterus may be prevented from being exhausted by the timely and *judicious* use of the forceps; he often gives ergot and applies the forceps also, but would be sorry to rely on ergot alone. Where there is nervous exhaustion he gives strychnia with the ergot. The placenta should be removed by well-directed pressure, not by traction on the cord. Dr. Atthill does not think that the administration of ergot will *per se* in the cases mentioned by Dr. Whittle certainly prevent hæmorrhage. Dr. Moorman (Nov. 15), whilst agreeing generally with the remarks of Drs. Whittle and Atthill as to the use of ergot and of the forceps, observes that when hæmorrhage is actually present, cold effusion often proves of great service. Dr. Bassett (Nov. 22) points out the advisability of preparing patients in whom the likelihood of post-partum hæmorrhage is believed in by a course of medical treatment, of which the administration of iron should form the basis. He insists on the importance of the introduction of the hand into the uterus as well as the employment of cold effusion. Dr. Howell recommends a uterine truss he has invented, with a spring of 7 lbs., one pad on the sacrum and one on the uterus. Dr. Musket injects cold water freely into the uterus, and gives ergot. (*British Medical Journal*, Sept., Oct., and Nov. 1873.)

Extracts from British and Foreign Journals.

Dilatation of the Os Uteri in Parturition.—Dr. Alexander Skene, the Professor of Gynæcology in Long Island College Hospital, New York, asks “what well-informed obstetrician of the present day would think of waiting for six hours or more after the head had descended to the floor of the pelvis, before using the forceps; or would delay forty-eight hours or more for the os to dilate, while the patient in the meantime was suffering the most distressing pain? It is for the purpose of overcoming the latter difficulty—one of the most important elements in the first stage of labour—that Dr. Skene submits some points regarding the process of dilatation of the os.” Dr. Skene then goes on to say, that the cause of the natural dilatation of the os is contraction of the longitudinal muscular fibres, which, by making traction on the os, opens it sufficiently to admit the bag of waters. The bag of waters is, however, itself an important agent in directly affecting dilatation, opening the os to the extent of two and a half or three inches before rupturing. There are many circumstances which may retard the process, and thereby prolong the labour, as well as add greatly to the patient's suffering. Prominent among the causes of delayed labour is *irregular muscular action*. A second is a rigid, unyielding condition of the os itself. Again, premature rupture of the membranes may occur, and we lose thereby the most important agent in dilatation. Or a tense condition of the membranes may prevent their bulging down into the os, and forming the wedge known as the bag of waters, or the head of the child may lie close down over the os, so as to prevent the liquor amnii from forcing down the membranes into the os. Another cause which prevents the completion of dilatation in some cases is obstruction to the head of the child in settling down into the pelvis. In all such cases the patients suffer long and severely, and the physician loses both time and patience in waiting for slow dilatation; and Dr. Skene thinks that much time can be saved, and suffering alleviated, by the judicious exercise of the “obstetric art.” The two principal methods of

dilating at present in use are by the finger, and by what is known as "Barnes' dilator," which is an india-rubber bag that can be filled with water. The first or manual dilatation is quite limited in its application, for the reason that little can be accomplished unless the os has dilated sufficiently to admit at least two fingers, and even then the pressure is irregular and inefficient. It ought, in fact, to be limited to those cases where there is an urgent necessity for rapid delivery, and when the os has begun to dilate, and is quite dilatable. Owing to the size and shape of Barnes' dilators, they are not easy to introduce until the os is dilated to the extent of an inch or more, and are not, therefore, applicable when the requirement for their use is greatest. Dr. Skene has conceived a modification of this instrument, which he has found easy of application. It consists of a hard rubber tube, about ten inches long, terminating in a bulb or knob at one end, having a stopcock at the other, and a curve to correspond with the axis of the pelvic excavation. The tube is about the size of a No. 9 catheter, and is only slightly flexible. It is carried into the india-rubber bag, and answers three important purposes. 1. It guides the introduction of the bag; 2. It holds it in place within the os uteri; and 3. It makes a convenient connection with the syringe used to fill the bag with water. When the parts do not yield to the hydrostatic dilator, Dr. Skene incises the edges of the os with an instrument he terms the uterine scarificator, which is a scissors-like instrument, one blade of which has a cutting edge, whilst the other is probe-pointed. (*American Journal of Obstetrics*, vol. vi., No. 1.)

Treatment of Gastralgia.—Dr. Joulin suggests a novel plan of treatment for this affection, which consists in the prudent employment of ice and of external revulsives. The general plan of procedure is—1. To apply an ice-bag or iced cloths for ten minutes, morning and evening, to the epigastric region. 2. To apply a mustard poultice immediately after the ice over the whole surface cooled by the ice. This is to be kept on as long as possible. 3. To take a spoonful of ice shredded into the form of snow, and mingled with a little sugar, morning and evening, every five minutes for an hour. 4. To be immersed three or four times a week in a warm bath containing two pounds of mustard. After the ice has been applied to the epigastrium for ten or twelve minutes the useful effect is produced, the skin is cold, rendered slightly anæsthetic, and the pains, when they are present, in general disappear immediately. M. Joulin for a long time used cold compresses renewed every two or three minutes, but their effect is much less energetic than that of the ice. The ice that is given internally should not be

allowed to melt in the mouth, for then only warm water is swallowed, whereas the object is to apply the cold directly to the walls of the stomach. In no uncomplicated case can harm accrue from this proceeding, but it should not be employed when the patient is suffering from cough or any chest affection. (*La France Médicale*, 1873, No. 45.)

Poisoning by Local Contact of Iodide of Mercury—Treatment by Strychnia and Electricity.—Dr. A. W. Foot gives an interesting case in his "Select Clinical Reports," showing the effects that may be produced by the local contact of mercury and a successful means of treating them. A herdsman, aged 23, of sound constitution, was admitted into the medical wards on the 10th April, 1871, with loss of motion and sensation in the right hand and fore-arm. On the 13th March, a little more than three weeks before admission, he had rubbed a quantity of red mercurial ointment, as cattle blister, into three head of cattle who were affected with pleuropneumonia. He had been in the habit of rubbing this ointment into cattle all through the winter, but on this particular occasion he neglected the precautions adopted on former occasions. He had been accustomed to cover his hand with a bladder, and afterwards wash the hand with soap and warm water; on this occasion the bladder became frayed away, and the lather of sweat and mercurial ointment came in direct contact with his hand; he did not think it worth his while to get a fresh bladder; he also on this occasion only rinsed his hand in cold water. Next morning, on waking, he found his right hand "numb-like, as if it was asleep;" as he went to his work across the hills he dipped it into a brook "to waken it up," and slapped it against his other hand, but it remained numb and powerless. He had two blisters applied, one above and another below the elbow, and then sought admission into hospital three weeks after the occurrence. The condition of the right hand was one of weakness rather than of complete paralysis, for although he could not "grip" with it, the dynamometer showed a power of $13\frac{1}{2}$ kilos. in the right hand, against 30 kilos. in the left hand. He could not flex, extend, abduct, or adduct the hand, he had a stinging sensation in the fingers, but the deltoid was not affected, nor the muscles of the upper arm; there was no atrophy. He was ordered iodide of potassium in five-grain doses twice a day, and subcutaneous injections of the liquor strychniæ, B.P., to be made in the right fore-arm; the $\frac{1}{18}$ of a grain was the amount of strychnia at first used, subsequently the $\frac{1}{32}$ every second day. On one day only was any tremor observed in the affected parts. 15th April: right hand = 14 kilos, left hand = 34 kilos. 22nd April: as he felt a pain in the right fore-arm from the elbow to the wrist, along

the course of the median nerve, the injections of strychnia were discontinued, and the primary induction current of Stöhrer's battery was from this date employed daily. He quickly improved under the use of this battery, and before long had perfectly recovered the full power and use of his hand and forearm. (*Dublin Journal of Medical Science*, Sept. 1, 1873.)

Physiological Action of Caffein.—M. Aubert shows that caffein, when injected in sufficient doses into frogs, rabbits, cats, and dogs, always produces exaltation of the reflex excitability and tetanus. It can be easily demonstrated that the tetanus originates in the spinal cord, and that the nerve trunks remain intact. It also produces a peculiar stiffness and rigidity of the extremities (in one instance M. Aubert observed in the frog that the muscles became white and opaque), which he thinks is due to the direct action of the caffein upon the muscular tissue. Its poisonous action may be overcome by the persistent employment of artificial respiration. Thus in a medium sized dog, 0.25 of a gramme of caffein may prove a fatal dose, but as much as 3 grammes may be administered to an animal of the same size and kind without death ensuing, if artificial respiration be steadily maintained. Caffein has not much action on the heart in frogs; the pulsations diminish in frequency, but they continue for some time after the animal is apparently dead. In rabbits it causes a remarkable acceleration of the pulse, with periodical intermission, during which the heart appears to be not tetanically contracted, but rather distended. In dogs the frequency of the pulse rises, whilst the blood-pressure diminishes considerably. The former effect M. Aubert attributes to the action of caffein on the excitatory apparatus of the heart, since the vagus is but little acted on. The diminution of blood-pressure is due, he thinks, to a more or less complete paralysis of the cardiac nerves arising from the cardiac ganglia. The action of caffein on man he finds not quite in accordance with the statements of others. Confusion in the head, increased rapidity of pulse, trembling of the hands, were the only symptoms he could observe after the administration of 0.5 gramme ($7\frac{1}{2}$ grains). In regard to the question whether the action of coffee infusion is exclusively referable to the caffein contained in it, M. Aubert states that both in the frog and in man the action of coffee infusion is far stronger than that of the corresponding doses of caffein, and that the extractive free from caffein is by no means destitute of action. The enlivening and cheering action of coffee is decidedly not possessed by caffein. (*Pflüger's Archiv*. 1872, p. 589; and *Centralblatt*, 1873, p. 124.)

Treatment of Chronic Hydrocephalus.—In a paper on the diseases of the brain and its membranes in children, by Dr.

C. R. Jordan, of Birmingham, in which he treats of chronic hydrocephalus, acute tubercular meningitis, coarse tubercular deposit in the brain, and abscess, the following observations are made in regard to the treatment of chronic hydrocephalus. If in chronic hydrocephalus an artificial cure is attempted, three rational modes of treatment recommend themselves to our notice: these are, first, the inunction of mercury or iodine, or both combined, into the scalp, so as to try and produce absorption of the fluid in the ventricles by their means; never in any of these chronic cases have good results been seen from this plan. Should anyone, however, wish to make the experiment, it is best to keep up the child's strength by the free use of cod-liver oil and the syrup of phosphate or iodide of iron at the same time, and also to add pressure as an adjunct; this pressure is the second plan attended to. It is said to be an imitation of nature's method of checking these cases. This is, however, scarcely true, since the effusion does not stop because the skull consolidates, but the skull becomes welded together when the effusion stops, and death, if it spring from the hydrocephalus itself, arises from the effusion taking place more rapidly than the growth and expansion of the skull permits, the brain becomes compressed, irritated, and to a certain degree inflamed in consequence, and death results. The treatment seems, therefore, rather to imitate the process of death than the process of cure. Yet, as it has seemed successful in some hands, it deserves notice; it was first advocated by Mr. Barnard, of Bath. The plan adopted by Trousseau is the best—to compress the skull by firm plasters, one from each mastoid process to the orbit of the opposite side, another from the nape of the neck to the root of the nose, a fourth crosses the head, and a fifth is wound firmly round it. Of course, such firm pressure requires careful watching, especially if the effusion is decidedly on the increase at the time of trial. This plan is also sometimes combined with the third mode of treatment, namely, that of evacuating the fluid by puncture; this is generally done gradually, in order to avoid taking the pressure from the brain too quickly; a small portion of the fluid is, therefore, drawn off at one time; the place for puncture generally chosen is the coronal suture, about an inch from the median line. This plan of treatment has three times fallen under Dr. Jordan's notice, but all three cases ended fatally. Yet there is no doubt that it has sometimes been successful. The theory obviously is that the capillaries of the walls of the ventricles and of the choroid plexus are more likely to recover their tone when the pressure from the fluid is somewhat abated, and this is to a certain extent true; but the analogy to pleurisy, where we have an elastic resilient lung and an elastic chest wall, is very imperfect; yet it must be

confessed that there is nothing to offer as a curative agent except one of these three modes. Dr. Jordan's own treatment is not to attempt a cure at all, but to give cod-liver oil, phosphate of iron, and other tonic remedies, leaving the recovery to nature; and this, perhaps, succeeds in quite as large a percentage of cases as any other plan. (*Birmingham Medical Review*, July 1873.)

Treatment of Erysipelas.—Dr. V. Kaczorowski reports upon a new mode of treating erysipelas, since the introduction of which into several of the hospitals at Posen no fatal case has occurred. The affected parts of the skin are gently painted over every three hours with a mixture containing one part of carbolic acid and three parts of oil of turpentine; and the same is rubbed rather more energetically into the adjoining parts. Compresses dipped in lead lotion are then applied, and, lastly, according to the extent of the disease, either an ice-bag is laid upon the part or the whole limb is wrapped round with cloths dipped in ice-cold water; and these must be industriously exchanged for fresh ones as soon as they become at all warm. When adynamic symptoms are present, Hungarian wine is given, and camphor mixture. Injections of morphia may be given, and care should be taken that the bowels are kept open. The parts where the acid and turpentine have been rubbed in become intensely red and even blistered; the epidermis, however, soon dries, and the skin appears as if tanned. In the course of twenty-four or at most of thirty-six hours, the progress of the disease is cut short, and the pulse falls. Relapses are not observed to occur when this treatment has been applied. (*Berliner Klin. Wochenschrift*, No. 53; and *Centralblatt*, 1873, p. 238.)

Treatment of Herpes Circinatus and Tonsurans.—In his article on herpes in the *Nouveau Dictionnaire de Médecine et de Chirurgie*, M. Hardy makes the following observations on the treatment of these affections. The parasitical nature of herpes circinatus and tonsurans indicates the line of treatment that must be adopted for their cure; the cutaneous eruptions, the changes that occur in the hair, and its falling off, are due to the presence of a fungus which must be destroyed. Sometimes before adopting this radical treatment, the inflammatory symptoms, which might be aggravated by the ordinary parasiticides, must be subdued by meal poultices and lotions of linseed tea or mallow infusion, by baths, and by some emollient and acidulated drinks. Local means for the destruction of the parasite may then be resorted to. In the case of herpes circinatus the best ointments are those containing sulphur or mercury; to an ointment containing from two to four parts of

sulphur in thirty parts of lard, M. Hardy adds one part of camphor. He finds still more useful the infraction of an ointment containing turpeth mineral, containing from one to two parts of sulphide of mercury in thirty parts of lard. Equally useful are the mercurial and citrine ointments, mingled with three or four parts of lard. M. Hardy has also employed ointments containing oil of cade, with tincture of iodine, with success. He has found lotions containing corrosive sublimate less serviceable. All these agents act by destroying the parasite, but to ensure success their use must be continued beyond the time when all traces of the disease have disappeared; if they be given up too soon relapses are very likely to occur. Sulphuretted and alkaline baths are valuable adjuncts to the treatment; the last should in particular be taken when the mercurials have been employed, in order to avoid discoloration of the skin, which is apt to occur if sulphuretted baths are taken. (*Nouveau Dictionnaire de Médecine et de Chirurgie*, tome xvii. 1873.)

Treatment of Cholera by Ferruginous Preparations.—

Cholera had scarcely shown itself at the mouth of the Seine when the Academy was overwhelmed with receipts for its cure. M. Burg has strongly advocated the value of copper and its salts; but iron has not been proposed till the following note was published by M. Robiquet. His theory of its value is based on analyses of the blood of patients suffering from cholera, and the following are some of the clinical facts with which he supports his views:—Called, he says, to attend a family, he found an unfortunate woman in the last stage, and dying. By her side was a little girl, aged 8, who had been seized six hours previously, and in whom the usual symptoms were well marked. M. Robiquet recommended—1. Alcoholic frictions over the whole body. 2. That the child should be wrapped up in a thick layer of warm wool. 3. That she should have a spoonful of hot soup every half-hour. 4. That at every alternate half-hour a spoonful of the following mixture should be administered:—Quinine wine, ʒiv. ; citrate of iron, 30 grains. In the course of an hour after this treatment had been commenced, the vomiting ceased and the pulse rose, and after three hours there was a good reaction established, and the little patient recovered speedily and completely. Five days subsequently the brother of the above child, aged 12 years, was attacked. The same treatment was adopted, but this time with less success; and M. Robiquet therefore substituted for the citrate of iron, iron reduced by hydrogen, suspended in syrup of quinine, in the proportion of three parts of the former to 200 of the latter. In two hours reaction commenced, and recovery followed. In another case, of a woman, aged 46, who suffered from the

so-called *cholera sec* or dry cholera, in which vomiting and purgation were absent, he adopted the use of the citrate of iron, and again found that it acted well, recovery following. M. Robiquet states that his experiences in 1849 with the ferruginous salts were very satisfactory, and that his results were fully borne out by his observations in 1866. (*Journal de Médecine*, tome xlv. Oct. 1873.)

Cerebro-spinal Fever.—Dr. I. Lewis Smith, in an instructive essay on an epidemic of this disease that has lately visited New York, remarks that there were an unusually large number of cases of pleuro-pneumonia and pharyngitis of an asthenic type, whilst many of the cases of pneumonia during the prevalence of the epidemic presented symptoms of greater gravity than usually accompany this form of inflammation. He does not think the disease emanates from the soil, nor that it proceeds from the diet, being spread over too wide an area and embracing all ages. It is not propagated by contagion. The attack usually commences in the early morning, and there is no premonitory stage. The patient experiences a chill or rigor, and this is followed by apathy and more or less profound stupor or delirium. Vomiting, with little apparent nausea, is an early sign. The temperature is usually elevated. Sharp pain in the head continues during the acute period of the malady. There is general hyperæsthesia of the skin. Some contraction of certain muscles or groups of muscles is present in all typical cases. Paralysis occasionally occurs, and is usually a late symptom. The pulse is always frequent. The prognosis should always be doubtful, the mortality being very great. The treatment may be divided into the preventive and the curative. The preventive treatment consists in the improvement of the general hygienic conditions in which the patient lives. The curative in the application of bladders of ice to the head and nucha to relieve the intense congestion of the cerebro-spinal axis. A hot mustard foot-bath or general warm bath with mustard should also be employed as early as possible. An enema to open the bowels is also proper. Bleeding in the form of leeches may be requisite in some cases. Some remedy is required which will diminish the capacity of the arterioles; and bromide of potassium is most in favour with the American physicians, a child of five years being ordered five grains every two, three, or four hours. The pain and excessive agitation sometimes present may be alleviated by opium or morphia. Quinine appears to be of little service. For the nausea there appears to be no better remedy than bismuth in large doses. Sustaining remedies should be commenced early, and good food and alcoholic stimulants given almost

throughout the application. (*American Journal of Medical Science*, Oct. 1873.)

Excessive Vomiting during Pregnancy.—Dr. Pallen suggests the advisability of, and has carried into practice, the induction of abortion in certain cases of excessive vomiting in pregnancy. The rules which guide him are that the patient shall be unable to retain anything on her stomach whatever, vomiting up even cold water; that she is daily losing flesh, showing that she really does not retain any portion of the food taken; that all the approved remedies shall have been tried and found inefficient; that there is insomnia and restlessness, with a pulse under 100. But if the pulse be 120 or more, and the patient suffer from tinnitus aurium, delirium, and dimness of vision, it is too late to perform the operation—she will die. He has performed it five times under these last-mentioned circumstances at the earnest request of friends, and the patients have all died. The cause of excessive vomiting, he thinks, is not, as Graily Hewitt supposes, retroversion of the uterus, but some disease of the womb, as granular erosion of the cervix, simple congestion of the neck, with enlargement or endocervicitis. If the excessive vomiting occur within sixteen weeks after impregnation, the entire ovum should be removed, without rupture of the membranes, by sweeping a uterine sound *around* it, so as to detach it from the decidua, carefully avoiding the rupture of the membranes. Afterwards the tincture of ergot may be given in such doses as can be retained. If the sixteenth week is passed, it is better to rupture the membranes, and trust to the action of the uterus afterwards. Excessive vomiting does not occur after the seventh month. (*St. Louis Med. and Surg. Journal*, Oct. 1873.)

Ozone in the Treatment of Disease.—Dr. Lender, at a recent meeting of the Gesellschaft für Heilkunde in Berlin, brought several cases before the Society in which the employment of ozone had proved exceedingly serviceable. Amongst these were—1. A case of rheumatic paralysis of the facial nerves, in which the local application of constant and of the induced currents of electricity had proved useless. 2. A case of the disturbance of the accommodation of the eye occurring after diphtheria, with inability to see near objects distinctly. 3. A case of effusion into the thoracic cavity after inflammation. At the same meeting, Dr. Lender exhibited some peroxide of hydrogen; and an excellent means of producing ozone in the sick-room, the compound consisting of potash, peroxide of manganese, and oxalic acid. (*Allgemeine Wien. Med. Zeitung*, No. 23, 1873.)

Treatment of Dysmenorrhœa.—In an address delivered at the opening of the obstetric section of the British Medical Association, Dr. George Kidd, of Dublin, after referring to the frequency of dysmenorrhœa, observes that there is at the present time a great tendency to refer all the symptoms to the condition of the uterus, and to neglect or ignore the influence of the ovaries. No doubt it is often due to some cause preventing the escape of the menstrual fluid from the uterus; the typical and most simple form of this class of cases is when the obstruction is produced by a small os uteri, and a narrow cervix. The condition of the os is here a congenital malformation; but it may also be an acquired condition, and is then the result of the contraction either of a cicatrix or of effused lymph. The impediment may, however, and often does, depend on other causes, such as a flexion, and then the symptoms may manifest themselves from the beginning of the menstrual life, or not till a later period. A polypus, especially if so situated as to cause a valve-like obstruction, or the growth of a fibrous tumour, or some form of inflammation, may also give rise to obstruction and dysmenorrhœa as an acquired disease. The pain in this form of dysmenorrhœa commences either when the discharge is beginning to flow or some time afterwards, and is paroxysmal in character, seeming to depend on the effort of the uterus to expel its contents. As soon as these efforts have so far overcome the obstruction as to allow the free escape of the discharge the pain ceases. Sterility is usually present. On examination, the impediment, its position and true nature, can be ascertained, and in the majority of cases it can be removed. Another form of the disease is always acquired. In it the pain begins a week or ten days, or more, before menstruation, and the breasts become painful and tender. The pains are aggravated when menstruation begins, and often continue throughout the whole period, but more frequently are relieved as soon as the discharge is established. Menstruation, in these cases, is often irregular and usually retarded, the discharge being scanty. This form is caused by subacute ovaritis. The treatment recommended by Dr. Kidd consists of leeching, especially at the anus, hot baths, hot syringing, sedatives to the rectum, counter-irritation over the ovaries, the internal use of the bromides, and above all rest, and especially physiological rest. (*Obstetrical Journal*, No. 7, Oct. 1873.)

Injections of Morphia in Hernia Incarcerata.—Dr. Alois v. Szatyory, of Lak, records three consecutive cases of incarcerated inguinal hernia, which all rapidly and perfectly recovered after the subcutaneous injection of morphia. The first of these cases was one of old standing. The patient, when M.

Szatyory was called to him, was lying down, with cold extremities and small pulse; there was disposition to vomit, hiccup, and violent pain extending from the naval to the inguinal canal. He was at once placed in a hot bath, and M. Szatyory made ineffectual attempts to replace the intestine at intervals for three hours. At length finding his efforts useless, he determined to perform herniotomy. Before doing so, however, he thought it would be expedient to relax the muscular walls of the canal by an injection of morphia solution of the strength of two grains to one drachm. Five drops of this were injected subcutaneously, and in the course of a few minutes the taxis was again attempted, when the intestine quickly and easily returned. Rapid recovery followed. The two other cases reported were equally successful. (*Allgemeine Med. Zeitung*, No. 17, 1873.)

Feeding with Peptones in Disease of the Stomach.—A recent number of the *Gazette Médicale* has an article on this subject. About two pounds of lean meat, chopped into pieces, are put into a china pan, with one litre of water containing 1000 of hydrochloric acid. The pan is then put into a Papins kettle, perfectly sealed, and subjected to maceration for about fifteen hours. The contents are then crushed in a mortar until they constitute an emulsion, and then put back into the kettle again for about fifteen hours. The substance thus obtained must be then completely neutralised with bicarbonate of soda, and evaporated afterwards to the consistence of pap. When thus prepared, this soluble meat is generally well accepted by patients. Milk and pounded biscuit may, however, be added for the sake of variety, and in order to avoid too great uniformity of food. (*Gazette Médicale*, and *Lancet*, Sept. 6, 1873.)

Conium in the Treatment of Insanity.—Dr. Kitchen, of the New York State Lunatic Asylum, states that during the last eighteen months he has been pursuing special investigations with conium in the treatment of insanity, aided by the thermometer and sphygmograph. The preparations used were the succus conii, an imported article, prepared by Ransom, Hitchen, and Co., and the fluid extract made by Squibb, of Brooklyn, New York. The dose of the succus which will produce the physiological action is from a drachm to an ounce, according to the motor activity of the patient, men requiring larger doses than women. The dose of the fluid extract is from twenty minims to a drachm. The succus is more palatable than the extract. From experiments on a healthy person it appears that moderate doses lower the temperature and pulse, without having any apparent effect on the respiration. The following physiological effects are observed in from ten to twenty-five minutes after a

full dose is taken. 1. Suffusion of the eyes and injection of the conjunctiva. 2. Giddiness, and sensation of weight along the orbit. 3. Dimness of vision and dilatation of the pupils. 4. Inability to mental effort. 5. Languor, muscular weakness, with a strong desire to assume a recumbent position. 6. A dragging sensation in the limbs. 7. Pulse and temperature lowered. 8. Gentle glow of perspiration over whole body. 9. Usually in half an hour the ordinary patient is asleep. Dr. Kitchen then proceeds to give a series of cases of different forms of insanity, in which conium acted extremely well. The following is one of them. A man, aged 21, single, farmer, uses tobacco, native of New York; no hereditary tendency to insanity; admitted to the asylum in January 1872. The patient was a large muscular man, and had enjoyed unusually good health till June 1871; whilst working in the harvest-field had partial sunstroke, from which he rallied and seemed to recover in about four weeks. In August following, symptoms of insanity were developed; complained of intense pain in his head, was unable to do any work; following this he became gloomy and despondent, was seclusive, refused to go to the table with the family, would not see his friends; was wakeful and restless; frequently sitting up almost all night. On admission, was thin in flesh, complexion sallow; pupils dilated; tongue coated; bowels constipated. Was put on bromide of potassium, at night, in doses of 20 grains. This was continued for two weeks, patient sleeping only a part of each night, and looking more haggard than when admitted. The bromide was then stopped, and fluid extract of conium was given in xx. doses at night, sleep followed, and he began to improve, became more cheerful, talked freely of his condition, and of the effect the medicine was having on him. Conium was continued till his discharge in April 1872. Patient gained twenty pounds in weight. Discharged recovered. (*American Journal of Insanity*, No. 4, 1873.)

Facial Paralysis.—Dr. C. Tryde observes that in facial paralysis sensory disturbances are often present, such for example as impairment of the sense of taste, numbness of the skin of the face. It is particularly noticeable that the feeling of pain on the application of electricity is usually diminished, seldom augmented. In rheumatic facial paralysis hyperæsthesia of the auditory nerve is common, whilst in other cases there is impairment of hearing, which he thinks is due to paralysis of the stapedius muscle. In one such case he soon obtained a cure by electrical treatment (the anode being placed on the ear and cathode peripherally). M. Tryde has observed on three occasions the coincidence of paralysis of the facial and herpes zoster, the lesion being probably propagated by anastomosis between

the cervical nerves and the facial. Cases of facial paralysis should be investigated with the aid of both induction and constant currents; the current in the latter case being alternately reversed. Partial recovery usually takes place, especially in the peripheric form, lacrymation and increased disposition on the part of the skin to sweat being common. Contractions indicate imperfect recovery, and, he thinks, do not occur in paralysis of peripheric origin; though this is not quite accurate, as Bettelheim has noticed at least one such case. Tryde has seen clonic spasms which may easily be confounded with convulsive tic in three cases. The explanation he gives of such spasms is that in consequence of partial paralysis a stronger effort of the will than natural has to be sent to the muscles. This impulse often does not reach the muscle or muscles for which it is intended, but is distributed to other muscles not required for the movements wanted. The prognosis is more favourable in paralysis of peripheric than of central origin. Of the former, those that have their seat in the *Canalis Fallopii* have a disposition to incomplete recovery. Suppuration in the ear is unfavourable. The more feebly the muscles respond to electricity the longer will the paralysis last. If the reaction to Faradaic electricity be only diminished, the paralysis will last from one to ten months; but if no contraction occur on the application of Faradaic electricity, only an incomplete cure will be obtained, and that scarcely in the course of a year. Where the reaction to electricity is natural, or but little impaired, recovery is rapid. When the case is of three months' duration no complete recovery can be expected. In his experience the recovery was later and slower in progress in proportion as the galvanic treatment was commenced late. In cases of moderate long standing Tryde applied galvanisation; in more recent ones, galvanisation and faradisation alternately; the change to faradisation is also indicated when improvement under the continuous current ceases. Persistent contractions are best treated with strong constant currents. (*Medicinisch Chir. Rundschau*, Band xiv. Heft ii.)

Therapeutic Value of Veratrine.—At the Congress of Bordeaux held in the autumn of last year, M. Bitot read a paper on the employment of veratria in cardio-vascular affections which have not yet reached the period of cachexia, and gave the following as the conclusions at which he had arrived:—1. Veratria is an important remedial agent in vascular disorders. 2. It is especially valuable in the disorders which are associated with functional hypertrophy of the heart. 3. As regards its action on the heart, contrary to that of digitalis, it is atonic and depressing. 4. In physiological doses it does not act injuriously like digitalis. When used for a long period consecutively, it

does not produce the same dangerous symptoms (it has no cumulative effect). 5. It appears to act as an indirect compensator. In augmenting the sensibility and mobility of animal life, it diminishes the morbid acting of the nervous system and of the contractile fibres of vegetative life. 6. Its action is very distinct from that of digitalis. When the last appears in operation the other should be used. 7. Like digitalis, its use is contra-indicated in the final stage of cardio-vascular disorders in asystolia. 8. It may be employed tentatively in all the diseases which affect the nervous system of vegetative life. (*Rapport du Congrès de Bordeaux*, 1872.)

Treatment of Acute Rheumatism by means of Firm Dressing.—This mode of treatment, recommended by Scutin and applied in three cases by Gottschalk in 1845, was forgotten, until again brought forward by Concato, and more recently by Heubner. It is the subject of a paper by W. Oehme in the *Archiv der Heilkunde*, whose observations have been made in *Wunderlich's Clinic*. The results of the applications of light bandages are—1. That the pains cease very soon after their application; the application consisting of pasteboard, occasionally, in children and patients in delirium, with a plaster of Paris bandage superimposed. As a rule, indeed, the pains cease in from twelve to forty-eight hours, or at latest after seventy-two hours. 2. The febrile symptoms are materially diminished in violence and shortened in duration. In a series of ninety cases, whilst the average duration of the febrile symptoms in the cases treated without the firm dressing, and for the most part with digitalis, was 9·3 days in hospital (+ 4·6 days before admission), those treated with firm dressing had a duration of 6·1 days. A fall of the temperature usually occurs after twenty-four or forty-eight hours after the application of the dressing. 3. The combination of the reduction of the pain and of the duration of the fever caused a shortening of the total duration of the disease, and hence acted prophylactically in preventing fixation of the joints. Lastly, the profuse sweats characteristic of the disease seem to be diminished. (*Archiv der Heilkunde*, 1873, xiv. p. 385.)

On Limiting the Motion of the Chest in certain Lung Affections.—Dr. M'Crea, of Belfast, remarks that considering how much value is attached to rest in the treatment of diseases of many other organs, it seems strange that no attempt is usually made to follow out this line of management in diseases of the lung. On the contrary, in certain affections of this organ, it is not unusual to advise an increase of exercise, and persons who are considered to have a tendency to phthisis are not unfrequently put through a course of athletics intended to develop the chest.

This has been sufficiently condemned by high professional authority, but the application of the principle of rest has stopped now. The attempt has not been made to moderate the ordinary and apparently necessary movements of respiration. And yet Dr. M'Crea thinks the injurious effects of constant motion rendered even more frequent than natural by the disease must be admitted. From this point of view he advocates the use of anodyne cough medicines, which in promoting rest of the organ have a utility beyond the mere allaying of the patient's discomfort, important though that object be. Dr. M'Crea has thought it most feasible to attempt the moderation of movement in limited local affections of small portions of the lung, such as we meet with in phthisis. It would appear, he thinks, from all we know, reasonable to believe that the healthy masses of lung would be able, without inconvenience and by a slight increase of activity, to compensate for any diminution or cessation of function that might be induced in the diseased portion. For the purpose of modifying the lung motion, hernia trusses, bandages, weights and special apparatus of various kinds suggested themselves, and were found, for various reasons, unsuitable. The greater number of his cases were observed in the Belfast Dispensary, and he had therefore no control over the patients, except on the occasion of their visits to himself. Popularity and the absence of irksomeness were, under these circumstances, most important desiderata in whatever mechanism might be adopted. Dr. M'Crea finally settled down to the use of the old-fashioned and popular emplastrum roborans, which was applied to the chest in various ways according to the seat of the disease. In affections of the apex he applies it in the following manner. Two strips of plaster, two to three inches broad, are required; one may be called the vertical, the other the horizontal strip. The former is applied up the front of the chest, over the clavicle, and down the back, and is applied first; the horizontal one passes under the axilla, going two or three inches beyond the sternum in front and the spine behind. Before the application of the strips, the patient is told to make a forced expiration, and the warmed plaster is immediately applied. The breathing must be limited, and no motion of the arm made for a few minutes after the application, in order that the plasters may have time to harden. Usually, great feeling of comfort is immediately experienced, and the cough is relieved. Dr. M'Crea gives the outlines of many cases successfully treated in this way. (*Dublin Medical Journal*, Nov. 1, 1873.)

The Treatment of Carcinoma Uteri.—R. Schröder gives the following details of the treatment he has adopted with

success in cases of cancer of the uterus. After removing all the disease that was recognisable as a distinct tumour, a deep hollow was burnt with the actual cautery into the part corresponding with the cicatrix. The cautery was pressed with firmness into all those parts where new growth could be perceived by the finger. After detachment of the eschar, the sound parts being guarded by the application of wool dipped in carbonate of soda, plugs of wool moistened with alcoholic solution of bromine (one part of the latter to five of the former) were pressed against the diseased parts for five to ten minutes. He is of opinion that the solution of bromine attacks the new growth more energetically than the normal tissue, and in two cases of which he records the details there was every reason for believing that a permanent cure had been effected. (*Sitz.-Ber. der physico-med. Soc. zu Erlangen*, 1873.)

The Treatment of Salivation by means of Atropine.—

Dr. Ebstein remarks that the inhibitory action that atropine exercises in experimental investigations upon the secretory activity of the salivary glands was very successfully applied to arrest salivation occurring in a man aged 68, who had an attack of hemiplegia on the left side. As one of the results abundant salivation took place from the left corner of the mouth. Dr. Ebstein ordered the daily administration of a pill, containing about $\frac{1}{8}$ of a grain of sulphate of atropine, and in the course of eight days augmented the quantity to three pills. The quantity of saliva, which had previously been from five to six hundred cubic centimetres in twenty-four hours, fell steadily to 300, then to 275, 100, and ultimately to 90 cc.; complete arrest was not produced by four pills. Ebstein now tried the effect of the subcutaneous injection of minute doses of the sulphate of atropine over the submaxillary glands, and was gratified to find that he could produce complete arrest extending from 4 P.M. to 6 A.M. on the following morning. The salivation then returned. Injection of the atropine into other parts of the body, as into the fore-arm, produced a similar effect, but more slowly. No inconvenience was felt except what arose from the dilatation of the pupils. The salivation here appears to have been due to irritation of the chorda tympani, and as Heidenheim has shown, atropine inhibits, prevents, or abolishes the action of the chorda on the gland, the circulation throughout remaining unchanged. (*Berliner Wochenschrift*, 1873, No. 25.)

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¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C. ; Williams and Norgate, of Henrietta Street, Covent Garden, W.C. ; or Baillière, of King William Street, Charing Cross.

Department of Public Health.

QUIXOTIC ETIOLOGY: DR. CUNNINGHAM ON CHOLERA IN NORTHERN INDIA, 1872.¹

THE Report of the Sanitary Commissioner with the Government in India on the cholera epidemic of 1872 in Northern India, if it should not ultimately attain any higher position, does certainly hold a foremost place among the curiosities of medical literature. For some time, in his annual reports, Dr. Cunningham, when discussing "questions relating to the etiology of cholera, has expressed opinions diverging widely from those commonly entertained in this country on the same subjects. In the present report he formulates his divergent views, and believes that he demonstrates, from the experience of the cholera epidemic of 1872 in Northern India, that the doctrines of the importability of cholera, and of the transmissibility of the disease through the medium of water, not only are disproved by observed facts in India, but also that the facts are such as equally show that there is no reasonable ground for their belief elsewhere. These doctrines, he holds, are the product of "theoretical discussions," not of "the patient investigation of facts,—of *all* the facts so far as they can be ascertained;" and "it is to India we must look for their ratification, for no country in the world presents such a field for the study of cholera as India, where one portion is the home of the disease, and others are subject to such severe and fatal miasmas" (sec. 3). Hitherto observers have not merely failed to record *all* facts

¹ "Report on the Cholera Epidemic of 1872 in Northern India." By J. M. Cunningham, M.D., Surgeon-Major Bengal Medical Service, Sanitary Commissioner with the Government in India. Calcutta, 1873.

but in particular they have disregarded *negative* facts, which, for purposes of investigation, are, according to Dr. Cunningham, "just as important and as necessary for forming a right conclusion" as *positive* facts (sec. 54); and the etiology of cholera, and indeed of other epidemic diseases, at least in the questions chiefly discussed, has become largely a question of "authority." "In approaching the difficult questions connected with any one of them, the first essential is to record the facts unswayed by prejudice and uninfluenced even by the opinions of the highest authority. This worship of authority, which is so common, is not altogether unproductive of mischief. One medical officer, for example, whom we may call A, meets no fact in support of contagion or the water theory, but he believes in the authorities who support these views, and he concludes that although he has failed to find any such evidence, B, C, D, and every other medical officer have been more fortunate. B does the same; so do C, D, and so on. The consequence is, that events are often viewed through an ideal, and therefore distorted medium, and much valuable advice is lost" (sec. 116).

Dr. Cunningham, happily placed for investigation, and holding that when we want to see our way in relation to cholera we are foiled by prejudice and authority, takes the observations made, as to that disease, in 1872, in Northern India, among 100 bodies of troops, and other communities, under careful medical supervision, and from these observations, given in the Appendix to the Report, checked by his own experience, he comes to the conclusions already stated. These conclusions, he admits, "virtually put us back to the same state of ignorance as we were in a hundred years ago;" but "if we are on a wrong road, the sooner we go back the better" (sec. 118). Now Dr. Cunningham has undertaken to prove that we are in the wrong road, and to put us in the right road, in the moderate compass of 150 pages folio, report and appendix. We should be grateful to him for the compactness of his proof, as for the magnitude of its consequences, if we could really repose faith in it. But difficulties arise to such reception from the very beginning. It does not beget confidence to find Dr. Cunningham (sec. 3) reiterating the popular fallacy, that because India is the home of cholera, it is therefore the place

where, above all, it may be most clearly studied. The statement betrays a want of comprehension of the difficulties under which cholera is studied in India, which seriously compromises the judgment of the writer. Dr. Cunningham's present report yields abundant evidence of the impossibility of studying exhaustively the etiology of cholera in the greater portion of the district to which his report refers; an impossibility resting upon the necessary ignorance of very much relating to the disease which goes on among the native populations. India may be the place where the pathology of the disease can be best, because most continuously, studied; but as to its movements and intimate etiology, these will, for a long time to come, admit of fullest observation among populations where the malady occurs at intervals, where a complete registration of the deaths and sickness from it is possible, and where its progress among all sections of society can be ascertained. What can be the value of negative evidence as to the importation of cholera (used in the English sense of the word) under conditions like the following, properly urged by Dr. Cunningham against internal quarantines?—"Hundreds of grass-cutters, and other servants, must go out and in (the cantonments) daily. Supplies must come from without. In many places the difficulty is increased by the fact that they lie, like Umballa or Norshara, on the great high road of communication. The mails must pass, all travellers cannot be stopped, nor can ordinary traffic be arrested without interfering with the whole trade of the country. And in addition to the number of people who must not be stopped, there are the many who cannot be stopped."

Dr. Cunningham rightly sees that the keystone of the etiology he attacks is the "contagiousness" of cholera. But this "contagiousness" he interprets in a way quite peculiar to himself. He treats of it from beginning to end as a question of personal contagion; of the transmissibility of the disease directly from individual to individual, a question never seriously entertained by any person worthy of attention in these later days. This amazing error runs through the whole of the report and the appendix. The question discussed by Dr. Cunningham is not the contagiousness of cholera in the sense understood by our European teachers of mark, but it is the question, Does cholera spread

like small-pox? "Cholera," writes Dr. Cunningham, "is supposed to be analogous to small-pox (?), and on this a superstructure is raised which has no real foundation. Small-pox, it is argued, is a contagious disease, and spreads only by contagion. Cholera belongs to the same class of contagious diseases, therefore it also spreads by contagion, and by contagion only. But do we know that small-pox spreads only by contagion? Its annual rise and fall in this country, recurring year after year, are facts altogether inexplicable on the doctrine of contagion, and prove incontestably that the law of contagion is not the law which governs the spread of small-pox" (sec. 117). "Again, it is the doctrine of contagion which has prevented progress. So long as men believe that all that is required to escape attack is to shun the sick, what interest can there be in sanitary progress (!)" (sec. 119).

This marvellous doctrine of contagion, developed wholly out of Dr. Cunningham's own consciousness, is that which is examined in reference to cholera in this report. The doctrine known in this country, in which the contagiousness of cholera is held to be peculiar to the disease, and to be developed under definite conditions, so far as this report is concerned might never have been enounced. It is with amazement we see that of this doctrine, which, as set forth in the masterly language of Mr. Simon, has done such enormous good in this country as directive both of sanitary and of scientific work, there does not appear to be any precise knowledge in Dr. Cunningham's report, and the report of his coadjutors, printed in the appendix. A Sancho-like reference to the doctrine is made in sections 66 and 89, but merely to serve as a foil to Dr. Cunningham's astounding imaginations. Quixotic-like, his whole argument as to the contagiousness of cholera is addressed to his own conceptions.

Similarly, in respect to the second great question, he raises the question of what he calls the "water theory." Dr. Cunningham is not dealing with the theory understood under that name in England, a theory applied to certain localisations of cholera. He treats of the theory which exists only in the minds of himself and certain other Indian epidemiologists. He treats of "the water theory irreconcilable with the geographical distribution of cholera!" (sec. 73). He writes of it as irreconcilable with the experience of "different camps, miles apart, supplied from alto-

gether independent sources, and yet suffering severely from cholera!" (sec. 77). All this is sheer nonsense. He is not a whit more happy in dealing with it in respect to localised outbreaks. He gravely instances scattered cases in a community as giving no support to it (*e.g.* of three cases in the Benares district jail, p. 41). And in one instance, the St. Peter's College, Agra, where an outbreak must have suggested to English observers a probability of dissemination through water, he writes:—

"The history of the very violent outbreak in St. Peter's College, the most severe ever known in the whole course of the epidemic, is just one of those cases which should be explicable on the water theory, if the water theory is correct. But it admits of no such explanation. The water used by the boys was drawn from a well common to the College and a considerable native community, among whom, so far as is known, ten cases occurred. *It may be argued that though nominally drawn from the same source, in reality it was very likely taken from the College well employed in former years—a well in most dangerous proximity to the old latrine, which is practically a well of filth close beside it.* But this explanation accords with the facts no better than the other, for the day boarders (twenty-seven in number) who drank of the same water as the boarders and orphans, and drank largely too—as I am assured, and as might naturally be expected, in that very hot weather—all escaped, with the exception of one. Anxious to ascertain the circumstances under which the boy was attacked, I found on inquiry that he was the only one of the day scholars who lived close by—a fact which points strongly to localisation, not to water" (sec. 87). Now, we learn from a report in the appendix that this last-named dangerous well had been regularly used for the College till within six weeks of the outbreak, and that the supply was then obtained from a well *a third of a mile away*. It is obvious from Dr. Cunningham's statement that water could be taken from the dangerous well at the time of the outbreak, if the servants had thought fit to do so; but in the detailed report there is not a trace of any inquiry such as might have been looked for, even on the vague doctrine of "impure water," as to the probable incidental use of the condemned water immediately preceding the outbreak. There were two sources

of water at the command of the servants of the College; but it does not follow, as Dr. Cunningham assumes, that if the bad source were incidentally used, it must be used by all persons frequenting the College alike.

Dr. Cunningham cites this instance among others as "strikingly supporting the idea of the localisation of cholera in particular localities, dependent not on water, but on *some other as yet unknown condition.*" He says not a word of the drainage of the College, and the detailed description of the outbreak in the appendix *actually does not furnish the necessary data on this subject!*

It is not requisite to follow Dr. Cunningham's report in detail, for his entire argument is based on a complete misapprehension as to the doctrine of the contagion of cholera, and of the transmission of the disease through water, as maintained in this country. But as further showing how much the report falls short of that scientific character, the want of which he has so freely condemned in others, we may note that Dr. Cunningham discusses the question of importation of cholera, without any reference to the important facts recorded in the appendix, that, with two or three exceptions, in all the districts under review cholera is present from year to year. In answer to the question, "Does cholera (since railways) travel any quicker now than it did before?" he gives a table of statistics from eight stations (Cawnpore, Allahabad, Agra, Meerut, Umballa, Kassonlie, Meean Mear) to show that "cases occur no earlier in the year than they did before." He makes no reference to the fact that, according to the table and the data in the appendix, no year, not even at Kassonlie, passes without cholera, and that the question of "travelling," as he puts it, could not have place with regard to any one of them.

"There is nothing more remarkable," writes Dr. Cunningham, "regarding cholera than its localisation in particular quarters of a place, and even in particular buildings." Abundant illustration of this fact is to be found in the report. In this respect, at least, cholera acts in India as it does in England. It is to such localisation that the doctrine of the peculiar contagiousness and mode of contagiousness held in England applies; and that the doctrine equally applies to India we

have not the least doubt. Neither have we any doubt, as this report most fully shows, that this doctrine and its important practical applications are to the present time gravely misapprehended in India. Dr. Cunningham's observations on the importance of thorough drainage and of a pure water supply read like a *reductio ad absurdum* after his discussion of the questions to which he has referred and the canons of observation he has laid down. If the negative facts are to be used as he would have them used, his opinions in these respects must have a like direction as his opinions on the contagiousness of cholera and on water as a cholera-carrier. Bad drainage and an impure water-supply are the common conditions of Indian towns, villages, and stations, but so is not cholera; and the case is infinitely stronger against the influence of these conditions (as Dr. Cunningham argues) than against contagion and water-carrying. No number of apparent negatives can outweigh a positive fact, and the single instance of the Theydon Bois outbreak in 1865, to which Dr. Cunningham does *not* refer, casts all his arguments to the winds in respect to the contagiousness of cholera and its transmissibility through water. It shows also in what consists the difference between bad drainage which is not a cause, and bad drainage which is a cause, of cholera; and between impure water which is not a cause, and impure water which is a cause, of cholera. It gives the clue, moreover, to the common causes of localisation of cholera in this country, and we apprehend that it equally affords a clue to the common causes of localisation in India. The earliest of the series of facts in the Theydon Bois outbreak—we have it virtually from Dr. Cunningham (sec. 89) himself—are liable to occur in every town, village, and station in India; the latter facts of the series, we have little doubt, will be found to follow upon the earlier when they are properly looked for. The facts of the Theydon Bois outbreak can be dealt with independently of any theory; and it is simply as a record of fact, and having regard to the magnitude of the interests at issue, that imposes the duty on every medical man in India to be prepared for their probable occurrence, and guard against such events. How little this common-sense view of the matter has been taken in India is painfully illustrated by the outbreak of cholera at

Secunderabad a short time ago, by the events at St. Peter's College recorded by Dr. Cunningham, and, generally, by his report and the reports contained in the appendix to it. It is hardly credible that, in the case of St. Peter's College, a well disused for pollution such as it was liable to should have been left so that water could be taken from it at the will of servants, who might dislike to seek a supply a third of a mile away. It is still less credible that the possible use of excrement-polluted water should not have been made the subject of immediate and most carefully conducted inquiry, of which there is no evidence. It is again incredible that such inquiry, having regard to the long admitted influence of impure water as fostering cholera, and that there was here question of water polluted in the foulest and most injurious manner, should seemingly have been made to depend upon belief or disbelief of a particular theory, or individual impression of a theory!

Dr. Cunningham rails against authority as influencing individual observation in sanitary matters. Dr. Cunningham's authority has gone forth against doctrines which, seen through ordinary spectacles, are the expression of well-ascertained facts. Let us hope that those who look to him as chief will not forget his utterance as to the mischievousness of authority-worship when they read this report. There are authorities and authorities; it is a strange spectacle to see the Sanitary Commissioner with the Government of India careering against mere figments of his own imagination. Dr. Cunningham's report is simply Quixotic.

ON "DISINFECTANTS."

(Continued from page 394.)

WE have recognised, as comprehended in the ordinary use of the term "disinfectants," agents operating in three quite different ways; and we have considered two of these classes of agents, those which operate essentially by promoting organic decomposition, and those which operate essentially by retarding such decomposition. We have endeavoured to show that agents of

these two classes have, so far as they ranged themselves under the one or the other class, certain kinds of usefulness to which plain but narrow limits had in practice to be assigned, and we contend that confusion and mischief only can arise by putting trust in these agents to influence, because they influence ordinary decomposition, the specific operation of infection also.

We do not expect to see the term "disinfectant" restricted to agents of the third class, to those which have this power of preventing the specific operation of infection; and we propose therefore to use the words "true disinfectant" to indicate those agents which, like sulphur applied to itch-clothes, will, without any primary or essential influence on processes of decomposition, prevent the communication of a specific disease from an affected to an unaffected person. Agents of this "true disinfecting" kind are probably no rarity: and their operation or their non-operation, under circumstances that are not recognised, is probably a foremost cause of the apparent anomalies in the spread of infectious diseases. Yet we know extremely little about them; and it is not to be supposed we shall ever know more, while we keep confounding them under a common name with agents operating by mere chemical destruction and with other substances that act as antiseptics,¹ retarding organic decomposition.

As illustrating what we are calling a true disinfection, we have chosen the power of sulphur to destroy itch-infection. Be it observed, however, that in the point of view for which we are contending, the itch-insect does not need to be recognised as the material of the itch-disease. Suppose we had never heard of an acarus, and that we had only a clinical knowledge of a

¹ It is curious to see another confusion of nomenclature in the writings of so prominent a worker in this field as Dr. John Dougall. His experiments, quoted further on in the text, as to the influence of certain gases and vapours on the efficacy of vaccine lymph, were made, he says, "to determine the relative *aërial antiseptic powers*" of those gases and vapours. By destruction of specific infectious quality he proposes to prove the power of an agent to prevent decomposition. It is true that, as the outcome of experiment, a majority of the vapours which spoiled vaccine lymph did also delay ordinary decomposition; and that a majority of the vapours which did not spoil vaccine lymph had little power of delaying ordinary decomposition. But even when this result is obtained (and even supposing it to have been more constant than it was), nothing is proved as to the identity of antiseptic powers with true disinfection.

popular and pustular disease, capable of spreading by contact and clothing from affected to unaffected persons, our propositions—that sulphur has a power to destroy this faculty of spread, and that sulphur has this power not by any essential influence on processes of decomposition, but by some special faculty of another kind—would remain true. And, in like manner, what we have to say about other agents of the "true disinfecting" class is, and for the present must be, quite irrespective of questions of intimate pathology. What we want to know is this:—Given the material, whatever it is, of an infectious disease, alone or mixed with other material, contained in the air, in clothing, or elsewhere; how can we render inert, and permanently inert, the power which the material possesses of causing the infectious disease in another body?

In the present state of our knowledge as to the nature of infection, an answer to this question can assuredly only be got by direct experiment and observation; not by inquiry about the influence of this or that agent upon "germ-life," but about its influence on the faculty of disease-convection from body to body. Dr. Henry in 1831¹ showed that dry vaccine lymph, heated for two hours to 140° or more Fahr., failed to produce the vaccine disease in children into whose arms it was inserted. Dr. Crace Calvert in 1871² finds that *germ-life in putrid albumen* is not completely destroyed at 300° Fahr., a temperature sufficient to injure cotton fabrics. Dr. Henry's experiment led him to advocate the use of heat-chambers to destroy the infection of disease. Dr. Calvert, on the strength of the above and similar experiments, believes "that no beneficial results can be obtained by the employment of public stoves as a means of destroying germ-life and contagion." It cannot be necessary to indicate the respective values of the two lines of inquiry. Yet every month we find writers, professing to deal with questions of contagion, proceeding on the method of which Dr. Calvert furnishes the most perverse example; while since Dr. Henry's time hardly anything has been done in the way of direct observation as to the effect of agents on the communicability of disease from one animal body to another.

¹ Phil. Mag., vol. x. pp. 363 and seq.

² Chemical News, Sept. 1871.

The line of direct inquiry instituted by Dr. Henry was commenced with a particular commercial object in view ; and, as furthering this particular object, he was satisfied with reaching certain results without attempting to be exhaustive. In a second paper,¹ Dr. Henry indicated fresh lines of observation respecting the use of heat as destroying infectious power. But, although he thought of chlorine and perhaps of other chemical substances, he did not conduct his own inquiry further.

Vaccine lymph has been, for obvious reasons, the chief infectious material upon which Dr. Henry and those who have followed his line of observation have made their experiments. By a series of trials with dried lymph, Dr. Henry found that 120° Fahr. was insufficient, but that a temperature of 140° Fahr. was sufficient to render such lymph of no effect when it was afterwards inserted into the arms of susceptible children. Dr. Henry's experience is in interesting accord with what is known of the effect of hot climates upon vaccine lymph.

It might have been supposed that, as questions arose as to the power of various chemical agents to prevent the spread of infection, the plain thing to do, in the first instance at any rate, was to make similar experiments with vaccine lymph mixed with these agents. This has, however, been done to a very limited extent, and, as it were, incidentally. Thus Dr. Dougall,² to whom we owe some of the most exact observations on the point, made his experiments on vaccine, only with volatile substances, and in the course of an interesting inquiry into what we cannot but regard as another subject. We have to thank him, however, for the knowledge that, of ten volatile substances, five, which had no common quality except that of conveying an acid reaction, succeeded in hindering, under the circumstances of his experiments, a mixture of vaccine lymph and glycerine from producing vaccinia when the mixture was inserted into the arms of susceptible children ; while the other five, none of which conveyed any acid reaction to a mixture of lymph and glycerine, did not hinder the lymph from producing its ordinary infective result. Carbolic acid stood in the latter category, that of inert substances : while agents so different as chloride of lime, sulphurous

¹ Phil. Mag., vol. xi. p. 22.

² *Glasgow Medical Journal*, Nov. 1872 and Feb. 1873.

acid and acetic acid, stood in the former category. It may be added that the conditions of the experiments were in all cases beyond any that could apply to aerial disinfection in the presence of living persons; and also that the experiments did not profess to determine whether the specific property of the lymph was permanently destroyed, or whether it was, under favourable circumstances, capable of redevelopment. Confirmatory but independent experiments on the effect of certain acid gases upon vaccine have been made by Prof. Cameron,¹ of Dublin.

Scarlatina virus was the subject of some, admittedly very incomplete, observations by Dr. Henry.² Having obtained infected articles of flannel clothing from persons suffering under this disease, he heated them through several hours to temperatures of about 200° Fahr., and caused them to be worn by persons who were presumably susceptible to scarlatina, and watched the result. All five persons on whom his experiments were made escaped altogether.

Contagious typhus, once experimented with in a similar manner, furnished Dr. Henry with a similar negative result.

Cattle plague infection, in its behaviour in presence of certain agents esteemed to be disinfectants, has been frequently observed with more or less accuracy. Mr. Crookes³ puts on record the following:—"A farm is chosen in the very hot-bed of cattle plague. The cattle on it are divided into two lots; 45 being placed in disinfected houses, and 28 in undisinfected open sheds. The disease is brought into each lot, on the same day, by direct inoculation of the virus. Of the disinfected animals only those actually inoculated fall a prey, whilst of those which are not protected by disinfection, the whole are rapidly swept off." The disinfecting agents here employed appear to have been carbolic acid and sulphurous acid.

Infective pus of various kinds, proved to be capable of producing in rabbits and dogs pyæmia and similar phenomena, has been treated with carbolic acid by Dr. Rosenbach,⁴ and the

¹ Report on Public Health, 1872.

² Phil. Mag., vol. xi., op. cit.

³ Third Report of Cattle Plague Commission, pp. 197, 198.

⁴ Untersuchungen über den Einfluss der Carbonsäure gegen das Zustandekommen der pyämischen und putriden Infection bei Thieren. Göttingen, 1872.

mixtures have been subcutaneously injected into those animals. The results, upon the infective quality of the several sorts of pus, of treating the pus in various ways with carbolic acid, varied with the kinds of pus and apparently with other circumstances, which are studied by Dr. Rosenbach, but which require still further study before the influence of carbolic acid in this respect, and the quantities in which it needs to be used, can be regarded as settled.

The above are all the observations, having any claim to exactness, with which we are acquainted respecting true disinfection. There are other statements to which no scientific importance attaches. We shall be glad if any of our readers, who know of other definite observations on the subject, will favour us with a reference to them.

The direction of further work upon the power of supposed disinfecting agents would appear to be sufficiently indicated by this commencement of work. All confusion of infectious property with microscopical characters must be resolutely set aside, as not to the purpose. Such relation as does actually exist between the two things is hitherto, in spite of much important study, too little understood for one to be taken as an index of the other. Scientific inquiry into the intimate nature of infection will, on the other hand, be assisted by any trustworthy independent facts about agents which hinder infective substances from producing their characteristic effects. For example, what are the physical differences between vaccine lymph remaining active after exposure to one high temperature, and the same lymph rendered inert by exposure to a temperature some score of degrees higher? We do not know whether Dr. Chauveau or Dr. Sanderson has investigated this point, but the answer to the question may be expected to throw some fresh light on the nature of the association which they have established between the particulate elements of vaccine lymph and the efficacy of the lymph.

Three lines of study, then, appear to deserve immediate and sustained attention.

1. Let us by all means exhaust the lessons that vaccine virus can give us. When the importance of the results to be gained

is understood as it should be, no difficulty should be allowed to stand in the way of experimenting with every supposed disinfectant substance upon the infectiveness of this particular virus. And there can be no great difficulty. There must be hundreds of public vaccinators with whom success in vaccination is a practical certainty. Let one such vaccinator observe the results upon his success of some one agent to which indisputably active lymph has been for various times, in various degrees, and under otherwise varying conditions, exposed; let another such vaccinator observe with another agent. Some few failures are the worst that can happen in each man's practice—failures that are capable of being remedied the next week. It would be well that the conditions of the exceptional vaccinations should be settled on some definite plan, and that their results should be recorded after some definite method; and from this point of view a general superintendence of the observations by the medical department of Government would be serviceable, though there is nothing in the project that requires it to wait for Government initiation.

2. Observations should be made on the infections of the lower animals. The best way of doing this would appear to be as follows:—A disease known to attack with definite and easily recognised symptoms some particular kind of animal, and known to spread from one animal to another by some particular secretion or discharge, should be chosen. This is, on various grounds, preferable to experimenting on animals with human infection or the infection of another sort of animal. The secretion or discharge should be exposed to various supposed disinfecting agents, and observations be made on the conditions under which, after such exposure, infective matter retains or loses its special power. Any coincidence of results in experiments on two of these infectious diseases, or any coincidence of result in these experiments on the lower animals with those made with vaccine in the human subject, will give most important aid towards general conclusions.

3. Though we may not see our way to making direct experiments on scarlatina, enteric fever, and other dangerous infectious diseases of the human subject, let it be remembered that proof of the power of reputed disinfectants over the spread of such

disease is not necessarily a matter of intentional experiment. The knowledge we possess respecting the means by which such diseases are propagated rests on observations of a multitude of unintentional experiments, and a road to knowledge of the same kind, in respect of the means by which such propagation may be hindered, lies open to anyone who has opportunities and qualifications for accurate observation of cases where reputed disinfectants are employed. The observation we have cited respecting cattle plague was an instance of an unintentional experiment, of a kind that (duly observed when it happens to be repeated) is calculated much to advance our knowledge. Country officers of health, dealing with first cases of imported infection, have especially good opportunity of such observation; and their reports could hardly contain any matter more valuable than the precise records of processes of disinfection, with their results, as applied to such cases; particularly if thoughtfully devised and duly obeyed instructions for disinfection have failed of their anticipated effect. Experiences of this kind ought of course to be gathered, collated, and made generally instructive. The Society of Medical Officers of Health would find here an admirable work for a standing committee. Or, an efficient medical department of Government would have, in such work, opportunity for "central" sanitary action to which no local authority would be likely to object.

COMPARATIVE STATISTICS: THE MORTALITY OF ENGLAND IN THE DECENNIAL PERIODS 1851-60 AND 1861-70.

A RECENTLY published Parliamentary Paper showing the *Average Annual Proportion of Deaths from specified causes at specified ages in England generally and in each Registration Division and Registration District during the decennial period 1861-70*, permits, for the first time, if we mistake not, an accurate comparison to be made of some of the more important causes

of mortality in the kingdom in successive defined periods of time. A similar Parliamentary Paper, with certain exceptions to be named presently, was published for the decennial period 1851-60. Both papers have been constructed upon a like plan from data collected in a similar manner, and the figures they contain are strictly comparable. The paper last published gives the deaths per 100,000 living of each of the following classes:—

1. *At all Ages*:—(a) from all causes; (b) from fever ("Typhus" of Registrar-General); (c) from diarrhœa, dysentery, and cholera; (d) from scarlatina; (e) from diphtheria.
2. *At less than One Year of Age* from all causes.
3. *At less than Five Years of Age*:—(a) from all causes; (b) from diarrhœa, dysentery, and cholera; (c) from diseases of the respiratory organs (excluding phthisis); (d) from diseases of the brain (including hydrocephalus); (e) from small-pox; (f) from scarlatina; (g) from measles; (h) from whooping-cough.
4. *At Ages between Fifteen and Fifty-five*:—(a) from phthisis pulmonalis; (b) from other diseases of respiratory organs, distinguishing in both classes males and females.
5. *At Ages between Thirty-five and Fifty-five*, from diseases of the brain (including hydrocephalus).

The paper for 1851-60, in addition to the details specified above, gave also the deaths of each of the several classes stated in three selected districts of least mortality; and also gave the deaths registered from "teething" at less than five years of age. It is not easy to surmise why these details should have been omitted in the recent paper, and it was certainly desirable, for the sake of maintaining the uniformity of the papers, that they should have been retained.

Papers such as these, consisting of masses of figures, are difficult for the reviewer to deal with. Their great value consists in this: that they furnish much-needed gauges of the mortality of different districts, invaluable for purposes of local investigation. We propose here, for the present, to state certain comparative results respecting England generally.

The deaths from all causes at all ages, in the proportion stated, were very slightly in excess in 1861-70 as compared with 1851-60, having been 2,242 in the first-named period, and 2,217 in the last. Of the specified diseases at all ages, scarlatina and diphtheria caused a larger mortality in 1861-70 than

in 1851-60, the mortality from "fever" being somewhat less, and that from diarrhoea, dysentery, and cholera the same. The deaths from all causes at less than *one* year of age, and at less than *five* years of age, were both in excess in 1861-70 as compared with 1851-60, as also were the deaths under five years of age from diarrhoea, dysentery, and cholera, from diseases of the respiratory organs, from scarlet fever, from measles, and from whooping cough; the deaths from diseases of the brain (including hydrocephalus) and from small-pox being alone below the number in 1851-60. The deaths, at ages between fifteen and fifty-five, from phthisis were less in 1861-70 than in 1851-60, both among males and females, but the deaths from other diseases of the respiratory organs were in excess in both sexes. The deaths from the diseases of the brain (including hydrocephalus), at ages between thirty-five and fifty-five, were also in excess in both sexes in 1861-70.

The distribution of the larger death-rates in 1861-70, among the different registration divisions,¹ is full of interest as suggestive of inquiry. Taking the first category of deaths at all ages, we find that the mortality, from all causes, was in excess in London, the North-Western Counties, Yorkshire, the Northern Counties, Monmouthshire, and Wales; from "fever," in the same divisions with the exception of Monmouthshire and Wales; from diarrhoea, dysentery, and cholera, in the South-Western Counties, the North-Midland, the North-Western, Yorkshire, Monmouthshire, and Wales; from scarlatina, in all the divisions except the South-Western Counties and the North-Midland Counties; and from diphtheria, in all the divisions except the last-named.

Of the second category, deaths from all causes at less than one year of age, these were in excess in London, the South-Eastern Counties, the West-Midland Counties, Yorkshire, and the Northern Counties.

Of the third category, deaths at less than five years of age,

¹ The following are the registration divisions:—1, London; 2, South-Eastern Counties; 3, South-Midland Counties; 4, Eastern Counties; 5, South-Western Counties; 6, East-Midland Counties; 7, North-Midland Counties; 8, North-Western Counties; 9, Yorkshire; 10, Northern Counties; 11, Monmouthshire and Wales.

these from all causes were in excess in London, the South-Eastern, South-Midland and North-Western Counties, and Yorkshire; from diarrhoea, dysentery, and cholera, in all the divisions; from diseases of the respiratory organs (excluding phthisis), in all the divisions except the North-Midland Counties; from diseases of the brain (including hydrocephalus), in the West-Midland and North-Western Counties, in Yorkshire, and the Northern Counties; from small-pox, in no division; from scarlatina, in all the divisions except the South-Western Counties and North-Western Counties; from measles, in London, the South-Eastern, South-Midland, Eastern, and South-Western, North-Western, and Northern; from whooping-cough, the South-Eastern, South-Western, West-Midland, and North-Western Counties, Yorkshire, Monmouthshire, and Wales.

Of the fourth category, at ages between fifteen and fifty-five, the deaths from phthisis pulmonalis were in excess among males in London, Yorkshire, Monmouthshire, and Wales; among females, in Monmouthshire and Wales only; and the deaths from other diseases of respiratory organs were, among males, in excess in every division, and among females in London, the West-Midland, and North-Western Counties, Yorkshire, the Northern Counties, and Monmouthshire and Wales.

Finally, of the fifth category, the deaths at ages between thirty-five and fifty-five, from diseases of the brain (including hydrocephalus), those among males were in excess in every division, among females in London, the South-Midland, South-Western, and North-Western Counties, Yorkshire, the Northern Counties, and Monmouthshire and Wales.

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